

Carving Up the Country

AFTER centuries of stagnation, there has in the past few years been a quite extraordinary amount of change in the basis on which the British government parcels out the country as a whole for purposes of local government. In the past few years, the ancient county of Middlesex is one of those that have disappeared altogether, largely by absorption into London. Now, under the provisions of the Local Government Act, the number of counties will be still further reduced, but the act also creates, for administrative purposes, new counties (such as the Isle of Wight) by fission of larger counties. It will be hard for future Shakespearian scholars for whom the names of counties stand for particular noblemen, to know quite what to make of upstart organizations such as Cleveland. Why no duke, they will want to know? But in the process of deciding just where local authorities' boundaries should lie, the British government has now gone one step further by publishing the report of a commission established nearly two years ago to decide how the counties themselves should be divided into smaller parts corresponding roughly to the rural district councils and the like which now look after local government outside metropolitan areas (*Local Government Boundary Commission for England Report No. 1*, HMSO, £1.30). The object of the exercise is straightforward enough. The government has been looking for local government units which are on the one hand big enough and rich enough to stand shoulder to shoulder with the towns which they frequently surround and at the same time sufficiently homogeneous among themselves for there to be no chance that some of the new administrative districts will be overshadowed by the rest.

On balance, the commission has done a reasonably good job in deciding just where the balance should be struck, even if it has not been able to do precisely what the government had asked—to devise a scheme in which there would be no local districts with populations smaller than 40,000. In the report, the commission suggests that there should be fourteen new districts with populations smaller than the limit originally aimed at by the government—plainly the lobbying of existing local authorities for their perpetuation has on the whole, in the past few months, been reasonably influential. But the commission has not given much ground—the number of new administrative districts has increased by only eighteen since the recommendations were first drafted, to a total of 296, which is itself much less than a third of the number of local authorities at present in existence. Obviously the new arrangement should be a considerable improvement on the present state of affairs.

That said, it remains something of a puzzle to know precisely what the local authorities are for. In future, as at present, county councils will be elected, as will the councils responsible for individual districts within a county, not to mention the city councils responsible to the government of metropolitan areas. County councils will share with local districts responsibility for certain aspects

of administration, education for example, as they have done in the past. In general, it is intended that the boundaries of the local authorities should be flexible, changing with time and with the shifting centre of gravity of the population. The weakness in this argument, of course, is that there is no necessary assurance that the process can be simple enough to provide the degree of numerical equality at, say, election time that makes for evident equality between districts. The Boundary Commission, in its report, asks that there should be continuing machinery for the adjustment of boundaries, at least in part with the objective of making sure that all administrative units are roughly the same in size. Given the movements of population which have been marked in the past few years, plainly the standing organization will have its work cut out. But is there not also a reason to strike a balance between the need for some semblance of equality between districts and the need for continuity? Indeed, it is exceedingly unfortunate that it has not been possible, even at the beginning of this new scheme, to link the definition of local authorities unambiguously with the definition of parliamentary constituencies, for in the long run that is one of the most powerful means by which a community acquires a sense of commonwealth and even a common sense of frustration.

In the long run, a more serious defect in the present proposals is that it still remains a matter of great ambiguity to know just which of the three tiers of government will be most influential in, say, matters of town and country planning. Indeed, even in the neighbourhood of the great cities, there has been comparatively little extra freedom for the would-be city planners. Yet for decades, people and organizations, many of them government committees, have been saying that regions such as the south-east of England should be planned as integrated wholes. Who in future is to say when the interests of the new districts should be overridden by the need for a larger plan, and when local autonomy should be most honoured? There is great good sense in the government's proposals for reorganization, which in any case have their roots in the previous government's term of office, but it remains the case that the kind of planning which is most important, that which determines the pattern of the country as a whole, is that which is most obviously neglected in the new arrangements.

Intelsat's Deadline

INTELSAT, the international consortium for the world's commercial communications satellites, finds itself racing the clock once again. By December 22, 54 countries must have approved the definitive arrangements for Intelsat, tortuously negotiated in three ponderous international conferences between 1969 and 1971, or the agreement will not come into effect. The State Department in Washington has been agitating, since the end of



the last conference in May 1971, for the 83 members of Intelsat under the temporary arrangements to produce their signatures to the permanent agreement. But it has been slow going. At the beginning of this week, six signatures were still lacking.

Why the reluctance to sign? It is the smaller countries which have been holding out. The accord of May 1971 stipulated that in order to take effect, the permanent arrangements for Intelsat must be ratified within 18 months by, first, two-thirds of the countries holding at least two-thirds of the investment shares in the satellite system as it exists now and, second, two-thirds of all the countries which were members of Intelsat in May 1971. The heavy investors in Intelsat, chiefly the United States, but Britain and Canada and Australia as well, signed straight away. They had no doubts about wanting Intelsat to continue as an organization which gave greatest influence to the heaviest users of satellite communication, like themselves (although the Americans were disappointed not to keep through Comsat, the private American satellite company, their overwhelming dominance of Intelsat). France held out a long while. It would have liked an Intelsat more favourable to Europe, but finally realized that there was no alternative. But smaller countries have been more emphatically unenthusiastic. They have little voice in Intelsat. They will be allowed to turn up every now and again to express their opinions in a kind of General Assembly but will have no real voice in what kinds of satellites are bought or what the rates are. That disenchantment (plus, to be sure, the slow process of having an international agreement ratified by 83 countries, each in its own chosen way) is what has dragged Intelsat down to the wire.

The deadline will probably be met and Intelsat will gradually assume the formal four-tiered structure agreed upon. If it is not, the 1971 agreement specifies that Intelsat will remain under its interim arrangements under which Comsat is both manager and majority shareholder. In theory, Comsat might not mind this state of affairs. But in fact it does not want to be seen to be in a muddle with its international business just as it is desperately trying to persuade the Federal Communications Commission to let it run at least one corner of an American domestic satellite service. If the accord of 1971 should collapse, then Intelsat itself might collapse. Or Comsat might be forced back into negotiations which would reduce its investment in Intelsat even further than the 1971 agreement.

Rutherford, Grand Old Man

THE *Notes and Records* of the Royal Society, usually a miscellaneous collection of jottings which will no doubt in future be less entertaining than they have been because of the death of Sir Harold Hartley, consist this current issue of a reprint of some of the papers read at some of the celebrations of the centenary of Rutherford's birth on October 28, 1971. At some point in the future, when people will be as concerned to understand why Rutherford was at once so successful and so influential as historians are now to know why Faraday made such a mark, this issue (*Notes and Records*, 27, 1; 1972) will be of some assistance, if only because it is more anecdotal than many of the accounts of Rutherford's life and work so far to

appear, even more rich in personal detail than Eve's biography (Cambridge University Press, 1939). By all appearances, it seems as if the Royal Society was able to persuade several of Rutherford's collaborators, initially his research students, to attend the centenary celebrations only if they were prepared to let their hair down. The result is that Lord Blackett, the most laconic of them, acknowledges the zeal with which Rutherford sought to keep his colleagues' energies concentrated on atomic nuclei, and his discontent when some even among the brighter of them chose to go off elsewhere. No doubt it is now beyond dispute that Rutherford was the kind of experimental physicist who can only previously have had one peer—Faraday—and it is no great surprise to find, in the lectures at the centenary celebration, that Rutherford emerges as a kind of curmudgeon among physicists, able to say that theoretical physicists "play games with their symbols but we in the Cavendish turn out the real facts of nature". To be sure, Sir Nevill Mott does his best to make Rutherford seem a little like the stereotype of the bullheaded experimentalist and he and other contributors to the celebrations may be right when they imply that the secret of Rutherford's success was not so much his indifference to theory as his flair for formulating his own personal representations of theory quickly and in a usable form. The truth is, of course, that there can be no field of science in which the progress of understanding can be held to depend exclusively on either experiment or theory.

100 Years Ago



The Greenwich Date

UNDER this heading, in your number for Nov. 28, a letter signed "James Pearson" ends thus:—"The query then is—in what part of the globe and in what meridian does October 20 end and October 21 begin?" As well ask where a circle ends and where it begins. See an article at the end of Bayle's Dictionary, entitled, in the second Rotterdam edition, 1702, "Dissertation sur le jour," vol. iii. p. 3118; in the London edition, 1741, "Dissertation concerning the Space of Time called Day," vol. x. p. 365. The difficulty, as Bayle shows, is in the nature of things. Let an equatorial railroad go round the world in twenty-four hours, with a station at every 45th meridian. At noon of October 20, Mr. West takes "a return ticket" westward; Mr. East takes one eastward. Both reckon by solar time. At every station Mr. West finds it noon, and on his return home reckons noon October 20; but the station-master reckons noon October 21. Mr. East at 45° sees the sun set at 6 o'clock. At 90° he finds midnight; at 135° the sun rises at 6 o'clock on October 21; at 180° it is noon. Here the two passengers pass each other, Mr. West reckoning it to be noon of October 20, Mr. East noon of October 21. At 135° W., Mr. East sees the sun set; at 90° he finds midnight; at 45° the sun rises at 6 o'clock on October 22. On his return home Mr. East reckons it to be noon of October 22. Here then are three different reckonings, and practically the keeping of Sunday, Christmas Day, &c., on different days in different countries exists at this instant and must exist for ever. Practically also those who sail eastward round the world get one more dinner than those who stay at home. Those who sail westward round the world get one dinner less than those who stay at home, and two dinners less than those who sail eastward, when both voyages are completed.

GEORGE GREENWOOD

Brookwood Park, Alresford, Nov. 30

From *Nature*, 7, 105, December 12, 1872.

OLD WORLD

Sir Alan Hodgkin calls for a Biological Centre

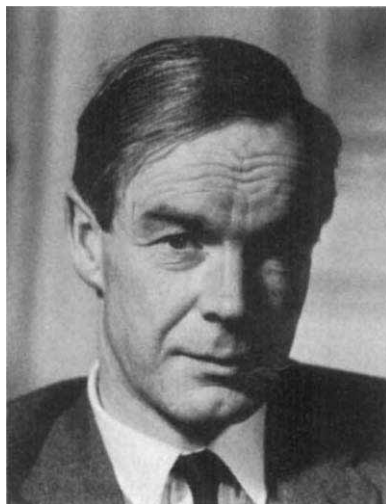
A CENTRE to supply biological material to research workers was called for by Sir Alan Hodgkin, President of the Royal Society, in his anniversary address last week.

The centre should supply biological and medical research workers with animals of all kinds, special chemicals found only in certain rare plants and animals, cultures of special cells and more common research aids such as labelled antibodies. Sir Alan made his proposal after pointing out that in physics the latest techniques are made available by laboratories such as CERN, Harwell and the Radiochemical Centre Ltd, but that there is no similar organization supplying biologists with the basic material for their work.

Sir Alan noted that although there are some centres supplying some biological material in Britain "the whole area is one which requires more thought and planning". And "quiet reflection about the changing needs of fundamental science" is something that has been lacking recently as scientists have been too concerned with the financing of research.

Sir Alan said that he did not wish to reopen the debate on the government's white paper on research and development, but he was worried about the effects of the white paper on the Natural Environment Research Council. Sir Alan is reasonably confident that the work of the Agricultural Research Council and Medical Research Council will survive the reorganization. NERC is relatively new and the £4.5 million that the council is losing under the government's plans will be split between several ministries, with the Department of the Environment and the Department of Trade and Industry getting the lion's share. "It is not obvious," Sir Alan said, "that either of these departments will feel the responsibility for keeping NERC's institutions financially healthy or for providing the expensive capital equipment—new ships for example—that is likely to be needed for marine biology and geophysics and oceanography."

The marine laboratories are major national assets, yet they do not fit easily or naturally into the customer-contractor relation and might be in serious difficulties under the new regime. "My feeling is," Sir Alan added, "that it will eventually be necessary to provide the research councils with new funds to replace those transferred to customer departments."



Sir Alan Hodgkin

Sir Alan thought it paradoxical that NERC should be in difficulties when it

covers the field of science that has made one of the largest contributions to Britain's prosperity. North Sea gas and oil may not have been discovered by NERC, but much of the basic research on the continental shelf came under NERC's wing.

On the subject of a centre for supplying biological material, Sir Alan also proposed that a European marine biological station be set up, possibly on the island of Madeira, which has good communications and an easy access to deep water.

Such centres, Sir Alan said, could provide much basic material for research, and he highlighted the point by referring to the advances in neurophysiology made from studies of *Aequorea forskalii*, a luminescent jellyfish, from work on tetrodotoxin taken from the Japanese puffer fish, and from the use of giant nerve fibres extracted from squid.

SOVIET SCIENCE

Medvedev for Mill Hill

THE way now seems clear for Dr Zhores Medvedev, the Soviet biologist, to spend some time in Britain pursuing his research. Following reports from Moscow during the past two weeks that he had been given a passport by the Soviet authorities, Dr R. Holliday, head of the genetics division at the National Institute for Medical Research at Mill Hill said this week that he expects Dr Medvedev to work in his group on the molecular aspects of ageing.

An invitation was extended to Dr Medvedev at the beginning of 1972 and the plans were that he would come to Mill Hill in January 1973. Dr Holliday said, however, that it is unlikely that Dr Medvedev will arrive by January—due to the late announcement of permission for him to leave the Soviet Union—but it is hoped that he will arrive early in the new year.

Dr Medvedev, who has often antagonized the Soviet government with his accusations that it hampered his contacts with foreign scientists, is likely to spend one year at the institute.

Medvedev's campaign for freer contacts between Soviet scientists and their colleagues abroad resulted in a manuscript which circulated among the liberal intelligentsia as a result of which he was dismissed in February, 1970 from his post as head of the



Dr Zhores Medvedev

Laboratory of Molecular Radiobiology at Obninsk. In May, 1970 he was arrested and detained in a mental hospital. A campaign of protest telegrams from leading Soviet scientists and intellectuals led to his examination by a special psychiatric commission from Moscow and his release after a further week of confinement. He was later allowed to continue his work and was appointed Senior Scientist at the Laboratory of Proteins and Amino Acids of the Research Institute of the Physiology and Biochemistry of Agricultural Animals.

SCIENCE CENTRE

Melbourne Experiment

Is there a need for a science centre in London similar to one which has been successfully operated in Melbourne, Australia, during the past five years? This is the question being asked by Professor A. R. Payne, director of the Shoe and Allied Trades Research Associations and professor at the Institute of Polymer Technology at Loughborough University.

The Melbourne science centre was discussed at the Royal Society of Arts this week by Mr J. E. Cummins, governor of the parent foundation. Since the centre was opened in 1968 with the objective of improving relations between scientific, technological and other related associations in Australia, it has gone from strength to strength and there are now eighty-two societies which are active participants. Thirty-four of these are accommodated within the centre and, according to Mr Cummins, this has resulted in much improved communication and understanding between the societies.

Added to all this, attendance at society meetings has increased and there has been an increase in interdisciplinary topics for discussion. Mr Cummins also predicted that one result of the close liaison between societies would be the amalgamation of small specialist societies with larger ones, of which they

would eventually form divisions. The chief advantage of this would be the saving of money on administration and in the publication of society journals.

One of the features of meetings held in the centre is that they are open to members of all other societies that go to make up the centre. But membership is not a formality and societies that wish to become members are first assessed to see whether they "are making a worthwhile contribution to the development of scientific research and technology".

Professor Payne sees the need for an applied science and engineering centre in London, based on the Melbourne concept, if only to provide a centre for meetings of the forty or so research associations. The centre would include secretarial staff and would provide meeting and lecture rooms.

The idea of having such a centre in London is new, according to Professor Payne, and at present he is only sounding out opinion among interested parties. But he says that there are many institutions, societies and collective bodies which would benefit from such a centre.

AGRICULTURAL RESEARCH COUNCIL

New Secretary Appointed

DR W. M. HENDERSON, director of the Agricultural Research Council's Institute for Research on Animal Diseases at Compton in Berkshire, is to be the new secretary of the ARC. The appointment, announced this week, comes a little more than a week after the death of Sir Ronald Baskett who had been the temporary secretary of the council since Sir Gordon Cox retired in 1971.

Dr Henderson, who is fifty-nine, has been at Compton since 1965 and director of that institute since 1966. He will take up his new appointment on December 11. No decision has yet been taken about who will succeed him as director of the Compton institute.



Dr W. M. Henderson, secretary-elect of the Agricultural Research Council.

BRITISH COUNCIL

Eyes on Europe

THE British Council, which exists "to promote a wider knowledge . . . of the English language, and to develop closer cultural relations . . . with foreign countries", is to devote more of its efforts to Western Europe in future. Although the council has traditionally concentrated on Africa and Asia, its report for 1971-72 says that "priorities and patterns of work cannot be immutable; they have inevitably to be adjusted to take account of political realities, changes in opportunities and financial circumstances".

In 1971-72 the council spent £16.9 million, but the estimate for 1972-73 is £19.0 million, of which £17.65 million is covered by government grants; the latter figure includes a special provision of £1.2 million to cover the cost of the council's increased European activities.

The council's own ideas have been backed up by agreements between the British and French governments to increase educational and cultural exchanges, and to this end the British government has given £100,000 a year to the council. And in March this year Mr Geoffrey Rippon announced that the government plans to spend an extra £6 million on the "further development of cultural relations with Europe".

The council also speaks in its report of the need to forge more links between British institutions of higher education, universities and polytechnics for example, and those in the rest of Europe. "A series of exploratory visits have been planned for specialists to identify opportunities for cooperation in the medical field and a similar reconnaissance has taken place in the sciences."

In its remarks on the teaching of science, the council says that new methods and new attitudes have aroused great interest in the developing countries. In particular, the ACTS scheme (Aid for Commonwealth Teaching of Science), which is financed by the Overseas Development Administration, now has people seconded to institutions overseas that are concerned with the methods of teaching science and mathematics and the content of courses. During 1971-72 the council placed 295 British teachers and lecturers in posts of all kinds at schools, teacher training colleges and universities abroad.

Interchanges of scientists arranged by the council are supplemented by extensive information services for scientists in foreign countries. The council draws particular attention to its Science and Technology Information Centre in Warsaw, which has been enlarged recently and now provides a British focal point for more than 200 Polish information centres, universities and research insti-

Memorial Lectureship

A FUND in memory of Professor Sir Ronald Nyholm, who was head of the department of chemistry at University College, London, until his death last December, is being set up by a group of his friends and the Chemical Society. The fund will be used to provide a medal and lectureship at University College and if the Royal Australian Chemical Institute's plans to raise money are also successful the lecturer will be able to deliver his talk in Australia, Professor Nyholm's place of birth, and to visit various chemical centres in that country.

The plans are still in embryo and the frequency of the lecture will depend on the money received by the organizers, who include Professor J. Lewis, University of Cambridge, Professor M. L. Tobe, University College, London, and Professor J. Chatt, University of Sussex. The organizers hope, however, that it will be possible for the lecture to be given every year.

tutes. The rapid and efficient service it can provide, says the council, is particularly useful in overcoming the shortage of material which results from the lack of foreign currency in Poland.

MEDICINE

Dentistry in the Doldrums

LESS attention should be given to transplant surgery and treatments for rare forms of cancer, and more to the problems of the elderly and the chronic sick, mental subnormality and in particular dentistry, according to Dr Henry Miller, vice-chancellor of the University of Newcastle upon Tyne, who addressed the Royal Society for the Encouragement of Arts this week.

Eighty per cent of British five-year-olds have some decayed teeth, 80 per cent of the population has unhealthy gums, half the population has lost its teeth by middle age and yet the cost of dentures in Britain is something like one seventh of that in, for example, Western Germany. "Melodramatic statements about the breakdown of the national health service have often been made without foundation, but there can be no doubt that the dental service has in practice broken down", concluded Dr Miller.

These are the areas of conspicuous inadequacy in an era of advanced medical care which, however, has not resulted in any diminution of the demand for treatment. Dr Miller has not been the only one to comment recently on the undiminished demand for the time of medical practitioners. Here as in the United States, the rise in the standard of general health has raised people's expectations and lowered their threshold for complaining.

Dr Miller does not feel that the general practitioner can any longer be expected to cope with the increasing need for specialized knowledge in medicine. He acknowledges that he is running against the tide of opinion in Britain, now the only advanced country to adhere to the general practitioner system, albeit increasingly streamlined by the formation of group practices within which some degree of specialization tends to develop.

No single person in Britain is responsible for seeing that there are enough doctors and nurses to staff the hospitals, and it is well known that without a steady influx of doctors from India and Pakistan, who make up 75 per cent of junior hospital staff, the hospital system would flounder. In this respect, said Dr Miller, we could learn from Eastern Europe where medical education is provided by state academies and it is the responsibility of the Health Minister to supply the demand for trained personnel.

Although hospitals have had a "lion's share" of health expenditure, said Dr Miller, they have suffered nevertheless from a need for large capital sums with which to replace antiquated buildings. He estimates that it will take fifty years to replace the present system, "by which time what is built now will be outdated". Furthermore the optimum size for a hospital is still controversial. It is simply not practical, Dr Miller pointed out, to institute a system of small hospitals each equipped with a different kind of specialized unit, when the increasing number of motor accidents will ensure that many patients will require the simultaneous facilities of several specialized units. The minimum economic size for a hospital able to supply this need would be about 2,000 beds.

The accident service is, in fact, one of the neglected areas which Dr Miller particularly emphasized. It deserves special attention because of the increasing number of traffic accidents, the very high proportion of young and active people affected by them, and the critical difference that prompt and appropriate treatment can often make. Prompt appropriate treatment implies large regional centres with competent 24-hour supervision.

STANDARDS

Centenary Ahead

from a Correspondent

MORE measurements of the density of water of different isotopic constitution was called for at the 61st session of the International Committee of Weights and Measures held in Sèvres, near Paris, recently. The conference also expressed the hope that the density of air, which is important in buoyancy correction in weighing and which has not been measured for several years, would soon be re-measured.

The international committee meets annually, and carries out the business of the Metre Convention in between meetings of the General Conference of Weights and Measures which take place every four years. The committee, as well as implementing the decisions of general conferences, supervises the work of the International Bureau of Weights and Measures (BIPM), sited at Sèvres.

The international committee is advised by a number of specialist consultative committees. The consultative committees for the definition of the second, for electricity, and for ionizing radiations have met since the 60th session of the international committee. The committee examined and discussed the reports of the consultative committees, as a result of which it recommended the use of the "international

CZECHOSLOVAKIA

Celebrations and Doubts

from a Correspondent

THE Twentieth-Anniversary Jubilee Meeting of the Czechoslovak Academy of Sciences was held in Prague on November 16 and 17, 1972. Reviewing the work of the academy since its foundation, the chairman, Academician J. Kožešnik, stated that the rate of expansion of science and technology in Czechoslovakia is such that the budgetary allocation is now some thirty times greater than before the Second World War.

The academy alone now comprises 143 research centres and employs some 14,000 persons. Academician Kožešnik laid special emphasis in his speech on the cooperation in basic research and technology between Czechoslovakia and the Soviet Union, represented at this meeting by Academician M. V. Keldysh, president of the Soviet Academy of Sciences.

Speaking on behalf of the government, Mr J. Foljčík, of the Central Committee of the Czechoslovak Communist Party, spoke of the need to develop further the role of pure and applied science in the national economy,

gravity standardization net 1971" already adopted by the International Union of Geodesy and Geophysics. In the past a value of the acceleration due to gravity, g , at Potsdam was accepted, and g at other places on the surface of the Earth was obtained by relative measurements; the value now adopted has been formed by weighting several absolute and relative measurements of g .

The centenary of the signing of the Metre Convention will be celebrated in 1975. BIPM will publish a book describing its activities to supplement one it published in connexion with the jubilee in 1925. The bureau will also prepare material for an exhibition to be held in Paris. The committee suggested that the opportunity should be taken to explain to the public in general, but more particularly to students and young scientists, the importance of metrology to society, industry, science and the economy of nations. It is planned that a short text on BIPM will be read in all French schools as part of the national celebrations. The committee hoped that the other member states of the Metre Convention would make similar arrangements, and requested BIPM to draft a letter to member states recalling the salient points of the history of the Metre Convention, pointing out that in 1975 the metric system would have gained almost universal acceptance.

but while his speech was generally congratulatory, there were certain undertones of what may be called echoes of 1968. His stand against "neutrality" among scientists, his urging that members of the academy should be at the same time committed to the aims of the Party, and his emphasis on the close integration of research with the development of socialist society tend to suggest that in spite of the cordial greetings from the Party on this jubilee occasion, the Czechoslovak Academy of Sciences is still causing the authorities a certain amount of unease.

EXHIBITION

Scientist for Mankind

THE current exhibition of the life and work of the late J. D. Bernal at the St Pancras Library, London, is an interesting but fragmentary tribute to this fascinating man and his many political and scientific interests. Born in Ireland at the turn of the century, he was aware from a very early age of the grave inequalities in human conditions. His concern with the role of science in society is well brought out in the exhibition, and his political interests, ranging from socialism in the 1920s to his involvement in the World Peace Movement and in the campaigns for nuclear disarmament in the 1950s and 1960s, are also clearly presented.

Professor Bernal was one of the founders of X-ray crystallography and he was one of the first to apply the technique to the study of biological molecules. In particular he is known for his work on the structure of liquid water, which was carried out in collaboration with his students and colleagues in the biomolecular group at Birkbeck College, University of London. This section of the exhibition, however, lacks coherence and it is a little difficult to follow chronologically. But the exhibition should certainly stimulate interest even in the non-scientist. Bernal possibly judged his political and social writings to be more important than his research: "The scientific work that I can do can be done and will be done by others but unless the political work is done there will be no science at all." His work for peace brought him into contact with many people outside the scientific and political communities.

A lighter side is provided by the comment of J. B. S. Haldane,

"Professor Bernal

Is not eternal

He may not escape from

The next bomb."

This was presumably prompted by Professor Bernal's amazingly accurate predictions of bomb damage during the war.

OPEN UNIVERSITY

Confidence Restored

DR W. L. M. PERRY, vice-chancellor of the Open University, is confident that the credibility of the university has not been damaged by the disclosure last week that eight part-time tutors had deliberately passed some students at a summer school because they disagreed with the examination. Dr Perry did add, however, that he was unable to comment in detail on the case until he had read the full report of the incident which he had not yet received.

The incident happened at a social science summer school where the students were set a one-hour general essay. The university emphasized, after the incident was disclosed to the public in *The Teacher*, that only 400 of the 5,000 students who are taking the

foundation course in social science attended this particular summer school and that there were only about 20 borderline cases. The examination papers of these students have now been re-marked.

The university emphasizes that the summer school grade is only used in the overall assessment of a student's work when the continuous assessment throughout the year and the result of the end of year examination are at variance. The university also points out that there are two external examiners for the social science foundation course.

The university, which relies on regular part-time tutors to provide an assessment of the work of its students during the year, keeps a close eye on these tutors through the senior staff who sometimes attend the tutorials and also approve the manuscripts which the past three tutors have marked.

MEDICAL CENTRE

Centre for Sussex

from a Correspondent

A PLANNED medical research centre at the University of Sussex will probably be the most ambitious venture in interdisciplinary activities yet to be undertaken by the university. The centre, which will be autonomous and will have its own building, will bring a variety of research activities under one roof. Plans for a 1,000-bed hospital to be built near the university campus have helped to influence the decision to set up the centre.

The importance of having a unified centre for medical research cannot be over-emphasized. It is more than 90 years since T. H. Huxley pleaded for close links between theoretical biology and practical medicine but the lesson, it seems, still has to be learned. Lord Zuckerman's recent comment on cancer research is sufficient evidence for this (see *Nature*, 240, 4; 1972).

A great deal of work on medicine is done at the university within the School of Biological Sciences. This work ranges from endocrinological studies to developmental genetics; from research on the behaviour of malarial mosquitoes to the mechanisms of cell division and the metabolism of cancer cells. Although the clinical application of this work is clear, the new centre will make communication easier between the practitioners and biologists.

Biomedical engineering is a thriving topic in the School of Applied Sciences at Sussex, and relations with doctors and surgeons are already established. A large part of the new centre's activities will be concerned with artificial heart valves, tubes with a range of intra-corporeal applications, such as oeso-

phageal replacement, studies on the feasibility of artificial fallopian tubes, life support systems and devices for vibration activated dilation — work at present undertaken in the biomedical engineering department.

Behavioural and medical research will be brought together in the new centre, probably to a greater extent than has been previously achieved. Questions about the socio-environmental and the behavioural aspects of patient care on the one hand, and those ethical considerations which are increasingly entering into medical discussions on the other, have usually been neglected. More often than not they have never even been appreciated. The Centre for Social Research at the university already investigates problems of the provision of health care, the study of psychosomatic factors in the aetiology of certain diseases and the relationship between the type of nursing and its effect on the psychiatric patient. These studies will be included in the new centre.

Complex ethical questions are now firmly embedded within the matrix of contemporary medicine, and a small international group, focusing on relevant ethical and legal questions, is already being coordinated from Sussex. This group has representatives from such disparate countries as the USA and Czechoslovakia, with the promise of additions from the Far East. Thus another research area in the centre will be examining ethical considerations as they relate to new progress in medicine and the life sciences.

The challenge of the centre is the promise of interesting work bearing on real, living problems, together with an opportunity to see just how successful such a multi-disciplined, and outwardly-orientated, group of people can be.

NEW WORLD

Murky Waters of Pollution Control

by our Washington Correspondent

WHEN Congress overrode President Nixon's veto of the Water Pollution Control Bill just before the election, it clearly won an important battle, but failed to win the war. Last week, the Administration announced that it would spend only 45 per cent of the funds that Congress had earmarked for construction of sewage treatment facilities this year and next. William D. Ruckelshaus, administrator of the Environmental Protection Agency, said that \$2,000 million would be spent this year and \$3,000 million in the 1974 fiscal year, which starts on July 1 next year—Congress had set aside \$5,000 million and \$6,000 million respectively.

The money will go to the states to help finance the building of secondary treatment plants. The water pollution bill specifies that every municipality in the United States must achieve at least secondary treatment by 1978, but Ruckelshaus said last week that no matter how much money is allocated to sewage treatment plants, such a goal is unattainable. The Administration has long maintained that even at the level of expenditure outlined in the bill, secondary treatment could not be made universal at least until 1981, and that the 45 per cent cut in funds will only delay attainment of the goal by about a year.

The Administration's action met with predictably strong words from Senator Edmund S. Muskie and Representative John A. Blatnik, chief sponsors of the water pollution bill. They accused the president of "flagrant disregard of the intent of Congress" and suggested that the Administration has only a "half-hearted commitment to the cause of clean water". But the Administration's critics have not been confined only to the Democratic camp, for there have been mutterings from Republican congressmen and last weekend Joel Broyhill, a colourful, conservative Congressman from Virginia openly wondered whether the reduced funds would be sufficient to meet the problem.

There is a chance, however, that some states will contest in the courts the President's authority to withhold funds allocated in the bill. Ruckelshaus told a news conference last week that the Administration decided that the grants to the states was the "soundest, most legally defensible place" to cut the funds, but he refused to speculate on whether or not some states would sue the government for the full amounts authorized in

the bill. At least the states can take heart from one thing, however; they will almost certainly receive considerably more than the Administration wanted to give them last year, when it proposed its own legislation for water pollution control, which would have led to expenditures of some \$6,000 million over three years.



William D. Ruckelshaus

CONSTANTS

Improving on Galileo

by our Washington Correspondent

THE National Bureau of Standards announced last month that laser beams have been used to measure the speed of light 100 times more accurately than previous techniques have allowed. The new value is given as $299,792.4562 \pm 11 \times 10^{-4} \text{ km s}^{-1}$. The determination is the culmination of some 334 years of attempts to refine the measurements, which started when Galileo tried to measure the speed with which a beam from a lantern crossed from one hilltop to another. His attempt failed. Although the accuracy of the new measurement will have only limited usefulness—chiefly in more accurate long distance measurements with lasers and radar measurements of interplanetary distances—the method by which it was achieved may have important applications in telecommunications.

In short, the new determination of the speed of light consisted of measuring the frequency of a helium-neon laser operating in the infrared, in terms of the caesium frequency standard, and the wavelength of a similar laser in terms of the krypton length standard. The speed of light was then determined by the product of the frequency and wave-

length. The key to the determination is a method, developed by scientists from NBS under the direction of Dr Kenneth M. Evenson, for stabilizing the fre-

President Resigns

by our Washington Correspondent

CLARENCE H. LINDER, president of the National Academy of Engineering, has announced that he will resign at the academy's annual meeting which will be held in May next year. Mr Linder is the first full-time president of the NAE, and he will be resigning after three years of a normal four-year term of office. The reason for his resignation, he says, is "because demands upon my time associated with personal responsibilities preclude continuing in the presidency beyond next spring". Mr Linder in fact made it known before he took office that he might retire before finishing his full presidential term.

Mr Linder's presidency has been marked by a series of negotiations and disagreements between the National Academy of Engineering and its parent body, the National Academy of Sciences. The NAE became an independent body in 1964, and it shares the resources of the National Research Council with the National Academy of Sciences. But the charter of the NAS gives the NAS council responsibility for the affairs of the National Research Council, thereby making the NAE a junior partner in the arrangement. The academy of engineering has always been resentful of its secondary role, and there have been persistent rumours during the past few years that it might set up shop on its own. The next president of the engineering academy will clearly have his work cut out.

Mr Linder's successor will be chosen by a nominating committee under the chairmanship of Mr Earl Cullum Jr. The committee will submit the name of a single nominee to the membership for mail ballot before the next annual meeting. In theory, other names can be proposed by the members at the annual meeting, but the chances of the nominating committee's candidate being turned down are remote.

quency of the laser. The beam is passed through methane gas and locked to the methane absorption line; another laser, using the same kind of absorption gas will then operate at the same frequency.

The improved frequency control made possible by this technique may open up new frequencies for telecommunications—perhaps even a thousand-fold increase over the number of frequency bands at present used for telecommunications. Another possible spin-off is that improved frequency control of tuned lasers will help the nascent science of monitoring atmospheric pollutants by lasers.

The accuracy of the determination also opens up the possibility of eliminating the need for a separate length standard. By defining the speed of light as $299,792,456.2 \text{ km s}^{-1}$, the length of a standard meter could be determined in terms of frequency. But to define absolutely the speed of light would require international acceptance.

ATOMIC ENERGY COMMISSION

Environmental Review

by our Washington Correspondent

THE Atomic Energy Commission has a tricky procedural question on its hands. Should the environmental review of individual power stations include the environmental effects associated with producing and fabricating the uranium fuel, reprocessing the spent fuel rods and getting rid of the radioactive wastes? Or should the fuel cycle be treated separately?

In several licensing hearings, participants have argued that the environmental statement should include an analysis of the costs associated with the uranium fuel cycle, but the Atomic Safety and Licensing Appeal Board has held that only the environmental effects associated with transporting fuel from the manufacturer to the reactor and irradiated fuels from the reactor to the reprocessing plant should be considered. The AEC has now, however, decided to hold public hearings on the question starting on February 1, 1973. The commission has also published for comment a survey of the environmental costs associated with the fuel cycle for a model light water plant producing 1,000 MWe. (Available from the Deputy Director for Fuels and Materials, Directorate of Licensing, US Atomic Energy Commission, Washington DC 20545.)

In general, the survey comes to the conclusion that the environmental costs associated with the fuel cycle for such a plant are so small that they would not significantly affect the conclusions from a cost benefit analysis for individual plants. Land use for the fuel cycle of a 1,000 MWe power plant is found to be less than 10 per cent of that required

to provide fuel for an equivalent coal-fired station, water usage for the fuel cycle is found to be less than 4 per cent of the total required to operate the plant itself, and the electrical requirements of the fuel cycle are reckoned to be less than 5 per cent of the energy produced by the reactor per year. As for effluents, the AEC suggests that perhaps the chief environmental impact may come from the emission of fluorine from the enrichment process, but the output of fluorine from the fuel cycles of 140 light water reactors would only amount to 0.2 per cent of the output of fluorine from other industries in 1980, the survey suggests.

As for radiological effluents, the most significant release is Krypton-85 from fuel reprocessing. The survey suggests that the annual radiation doses to an individual at the boundary of a reprocessing plant would amount to about 0.8 mrem to the whole body, 1.0 mrem to the bone, 15.5 mrem to the thyroid and 14 mrem to the outer layer of skin. The thyroid dose is about 1 per cent of the guideline limits recommended by the International Commission on Radiological Protection.

The survey makes no recommendations, but the implication is that the AEC considers that the environmental effects associated with the fuel cycle are so small that they need not be considered in individual applications. On the other hand, however, the survey gives no reasons why all the environmental costs of the operation of a power plant should not be considered in the environmental review.

UPPER ATMOSPHERE

German Satellite

by our Washington Correspondent

A GERMAN scientific satellite, AEROS, will be launched next week by NASA from the Western Test Range in California. Designed to study the upper atmosphere, the satellite will be placed into a polar orbit with an apogee of 800 km and a perigee which will be as low as 230 km. This highly eccentric orbit will allow the spacecraft to dip into the upper atmosphere to sample the chemical and electrical composition of the upper atmosphere and the F region of the ionosphere.

A cooperative project between NASA and the Bundesministerium für Bildung und Wissenschaft (BMBW) of the Federal Republic of Germany, AEROS will carry five experiments—four German and one developed at the Goddard Space Flight Center. The four German experiments are a mass spectrometer to study the chemical composition of the ambient atmosphere and ionosphere, an experiment to determine the electron energy distribution and ion tempera-

ture, an impedance probe to measure electron density and an extreme ultraviolet spectrometer to measure the intensity of the solar radiation. The US experiment will measure the temperature and the overall density of the neutral ambient atmosphere. One objective of the mission will be to correlate the data on the ionosphere and ambient atmosphere with the observations of the solar radiation, which ionizes the upper atmosphere.

Air friction in the upper atmosphere will gradually lower the perigee and apogee so that after about 130 days, the perigee is expected to decrease to about 220 km and apogee to about 580 km. Small hydrazine engines on the spacecraft can then be fired to increase the apogee and extend the life of the spacecraft. The atmospheric density at perigee will also be calculated from the orbital decay, by measuring the rate of change of the orbital period.

Short Notes

Space Tug

THE National Aeronautics and Space Administration has asked for proposals from the aerospace industry on the study of space tug systems. The tug would provide additional propulsion for payloads launched by the shuttle. NASA is asking for proposals for three study contracts, each worth \$750,000, two of which would examine the feasibility of a tug fuelled by liquid hydrogen and liquid oxygen, and the other would study a tug using storable propellants. The tug would be fuelled on the ground, launched by the shuttle in low earth orbit, recaptured by the shuttle and returned to Earth for repeated use.

Weizmann Institute

THE Weizmann Institute of Science in Rehovot, Israel, will get a share of the National Cancer Institute's budget for cancer research. It was announced last week that the NCI has awarded a contract worth \$447,000 to the Weizmann Institute to set up a multi-disciplinary cancer research programme. Part of the money will go to Dr Michael Feldman for a study of the effects of both thymus-derived and bone-marrow-derived lymphocytes on the growth of cancers in experimental animals. Dr Leo Sachs will receive money for the study of mechanisms for controlling the growth of mouse leukemia cells, Dr Michael Sela will work on the role of antigens as a means of detecting cancer, and the remainder of the money will be spent on a study directed by Dr Nathan Trainin which will also be concerned with the effects of lymphocytes from the thymus on the growth of cancer cells.

NEWS AND VIEWS

Persistent Pesticides and PCBs in the Environment

IN view of the controversy in recent issues of *Nature* about the relationship between organochlorine residues and eggshell thinning (see, for example, *Nature*, **240**, 162-166; 1972), now is an opportune time to put the problem in its context.

Organochlorine insecticides, such as DDT and dieldrin, have been used extensively in agriculture, forestry, industry and for the control of disease vectors for more than twenty years. The chemically related polychlorinated biphenyls (PCBs) were invented earlier, but their extensive use in plastics, paints, insulating oils and other industrial products coincides with the use of organochlorine insecticides. Both types of substances persist in the environment for much longer than most pesticides and they also share the common feature of great fat solubility which causes them to be stored in lipids which, in turn, enables them to be transferred from one organism to another. The debate about their biological effects is important because these substances are very effective, at least in the short term, but at the same time they appear to threaten populations of fish and other species of wildlife important to man. It is regrettable, but perhaps not surprising, that the scientific problem of their immensely complex effects has become confused at times by interposed value judgments and that it is even deliberately obscured by powerful vested interests.

Three kinds of question have been raised about the significance of these substances in the environment. First, persistent organochlorine insecticides and their stable metabolites and PCBs have been reported in organisms from many parts of the Earth. Is there any doubt about their identification? Before the recognition of PCBs and the proper use of confirmatory techniques, errors in identifying organochlorine insecticides were possible, but today there is general agreement that both the pesticides and the PCBs can be correctly identified, although there are still considerable difficulties in quantifying small amounts of PCBs in samples from the field. There is now no doubt that both persistent organochlorine insecticides and PCBs are very widely distributed in water, soils and organisms throughout the world.

The second question is whether these substances are biologically active at the low concentrations which are found in the field. The amounts of them in many organisms are so small that it is certain that they can have no acute effect. Their general significance thus largely depends on whether they can have biological effects at doses considerably smaller than those which are lethal: this can be determined by toxicological experiments.

Most of the earlier research was carried out on acute oral and dermal toxicity, and very little on sub-acute effects, but in recent years experimental studies have shown that persistent organochlorine insecticides and PCBs both have a wide range of sub-acute effects at doses considerably below the acute ones; these include effects on enzyme induction, fine structure, metabolism, reproduction and behaviour. There seems to be considerable variation in sensitivity to the same chemical between different species. The significance of sub-lethal effects in the field has been disputed, but disagreements about

laboratory experiments are relatively trivial. There is general agreement that in the laboratory persistent organochlorine insecticides and PCBs can have measurable effects at dose rates considerably below those which are lethal. Whether these effects occur in the field and are biologically important are other matters.

The third question concerns these matters and is ecological and fundamental. Do persistent organochlorine insecticides and PCBs significantly affect populations of organisms other than those in the target areas or those subjected to accidental or deliberate release of concentrates? The points raised by asking this question are particularly interesting because they involve an appraisal of the nature of scientific evidence. Situations in the field are always complicated, factors interact and a population increase or decline may be caused by one factor in one place and by another in another place or at another time. A sub-lethal effect may be critical in one case but not in another. Each case is thus separate and must be considered on its merits. Studies on British birds have provided good examples; eggshell thinning and egg breakage associated with large residues of organochlorine insecticides have been reported in the heron, golden eagle, peregrine and sparrowhawk. The peregrine and sparrowhawk have also been affected by acute toxicity of these substances. Acute toxicity rather than sub-lethal effects was found to be the principal cause of the population declines of the peregrine and sparrowhawk. The population of the golden eagle was potentially endangered by sub-lethal effects, because these would have prevented the replacement of individuals lost by age and predation in much of Scotland; by contrast, sub-lethal effects did not prevent replacement in the heron and this species was not endangered by them. It seems that the population of one of the American races of the peregrine (*Falco peregrinus anatum*) was affected by substantial exposure to DDT throughout its range rather than by the acute effects of dieldrin, as was the case in Britain.

For many populations and species, especially the less abundant vertebrates, it is virtually impossible to carry out the crucial field experiment to determine whether the pesticide does or does not affect the population. This has forced biologists to use circumstantial evidence in order to arrive at conclusions. Other biologists, who can solve their problems by experiments, often have little sympathy for those who are forced to use other methods, and perhaps too readily dismiss conclusions based upon the critical use of observations. The argument that if you cannot do a controlled experiment, any hypothesis is as good as another, is, however, unscientific and contrary to commonsense.

The principal objection to the use of circumstantial evidence is that the case seems to rest on *post hoc propter hoc* arguments: the decline in the bird of prey population is noted after the introduction of the chemical and is then attributed to it. If conclusions were based entirely on correlations of this kind, they would have very little value, but if other relevant information is taken fully into account—notably detailed consideration of alternative hypotheses—knowledge of the biology of the species and

of the toxicology of the chemical to it or closely related species, one hypothesis can be shown to be greatly preferable to others. In the absence of the crucial experiment this is as far as the scientist can go, but the element of doubt can be extremely small. The situation is a familiar one in ecological research.

The present debate about the effects of persistent organochlorine insecticides on eggshell thinning—a phenomenon reported from Britain, Continental Europe and North America—demonstrates the need to take all the relevant information into account before coming to a conclusion: unfortunately some workers have signally failed to do this.

Partial or total restrictions on the use of the pesticides provide a type of circumstantial evidence which is particularly valuable because they provide a further means of testing the hypothesis. If biologists were right in saying that the declines in reproductive capacity and of populations of birds of prey were caused by persistent organochlorine insecticides, breeding success and populations should recover after the introduction of bans on the use of the insecticides. The case of the golden eagle in Great Britain is a good example. In this species eggshell thinning and reproductive decline in western Scotland were associated with the use of dieldrin as a sheep dip: four years after the withdrawal of dieldrin for this purpose, eggshells and reproductive success had returned to normal and were similar to those of golden eagles in eastern Scotland where they did not feed on carrion sheep and so had never been affected by dieldrin.

There is very good evidence that the reproductive capacity and population size of some birds of prey have been severely affected by organochlorine insecticides. The evidence cannot be absolutely conclusive, because it is theoretically possible that some unknown substance, with exactly the same temporal and spatial distribution as the organochlorine insecticides, has been the cause. The evidence is accepted by a wide range of workers in many disciplines and by almost all who have first-hand knowledge of bird populations. No serious alternative hypothesis has been advanced to account for the remarkable phenomena observed in the field. The fate of birds of prey is the proper concern of conservation biologists, but its indication of an important new problem is of general concern.

Administrative action on the control of pesticides and pollutants has to depend on the scientific information available. The situation is familiar enough in the study of human disease and the effects of drugs: experiments on non-human animals are extrapolated to man, and evidence from epidemiology and case histories is critically reviewed. The same method should be used in assessing the effects of pollutants on wildlife. Value judgments have affected the criteria used to assess the scientific evidence on pesticide effects: a degree of certainty is demanded about effects on wildlife which is not demanded in similar situations in medical or agricultural research. Administrative action must be based on value judgments, but these should not be used in determining the validity of scientific evidence.

The decision by the Cook Committee in 1964 to reduce the use of persistent organochlorine insecticides in the United Kingdom was based primarily on the knowledge that these substances were widely present in the environment and that they could damage non-target organisms. The information obtained since 1964 has confirmed the

wisdom of the original decision. There is no evidence that PCBs have yet seriously damaged any wild species; nevertheless when it became known that they were widely distributed in terrestrial, freshwater and marine organisms and that they were biologically active at low concentrations, Monsanto withdrew those uses of PCBs which could contaminate the environment in the United Kingdom and later did so in the United States. A parallel with the organochlorine situation had been drawn and had been acted upon.

The extent to which a country can limit the use of persistent organochlorine insecticides depends on local conditions. The need for at least some of their uses in tropical countries will probably remain for some time, but their total or partial withdrawal in nations in the temperate zone is a prudent precaution to protect natural resources. The real argument about organochlorine insecticides is not whether one should prefer men to peregrines, but whether men should take serious note of what has happened to peregrines as the result of using substances which are both valuable and at the same time dangerous to a wide range of organisms.—N. W. M.

Dust in the Atmosphere

Two articles in this issue of *Nature* (pages 345 and 347) are concerned with techniques for tracing and measuring concentrations of dust in the lower atmosphere. Only a few years ago, for the first time, samples of dust from a volcanic eruption near the equator were collected in the stratosphere by aircraft over Australia (*Nature*, **203**, 824; 1964), and observations of radiation at the surface were used to detect the arrival of part of the same dust cloud over the South Pole.

In the article of Larson *et al.* the dust concerned was apparently typical of that which is picked up by air masses passing over continents, and the history of the air mass was verified by the use of other gaseous tracers—the naturally occurring, chemically inert gas radon (^{222}Rn) and methane (CH_4) as well as carbon monoxide which probably signifies man-made pollution. The other article, by Rosen *et al.*, is concerned with the dust thrown up to a maximum height of 8 km by the small eruption of the volcano Alaid in the Kurile Islands. A capacity to monitor the atmospheric spread of dust clouds, particles and foreign gases from all sources is essential before the effects upon climate, vegetation and the human and animal populations can be surveyed.

Human activity is, of course, very far from being the only source of particulate matter, noxious gases or other harmful substances in the atmosphere. Great numbers of sheep and cattle were killed in Iceland by fluoride poisoning of the grass after the eruption of Hekla in 1970, and that was only a small-scale repetition of similar unhappy events earlier in that country's history of natural disasters. In 1783 there were also human deaths; the ash fall reached Scotland and the smell of sulphurous gases was quite widely reported in Europe. But man-made toxins and man-made particles do now account for a large, and increasing, share of the total, most of the rest of the solid matter being desert sand, soil particles and sea salt. A recent estimate of the amounts of particulate matter taken up each year by

the atmosphere over the northern hemisphere takes the quantity of natural particles as a steady 690 million tons (110 million tons from oceans and 580 million tons from land surfaces); added to this is a man-made contribution, put at 480 million tons today, compared with 120 million tons back in 1880 and a predicted 760 million tons by the year 2000. Amidst the justifiable concern about possible implications of this trend, there has been much jumping to conclusions and a general tendency to overlook, or ignore, the incidence of natural fluctuations and trends in climate.

Excessively confident voices among scientists in the United States and Russia, and some leaders of opinion elsewhere, have proclaimed in recent years that it is the increasing incidence of man-made dust in the atmosphere that accounts for the change in climatic trends since 1950. Before about that time a global warming was in progress which could be traced back as early as the beginnings of the industrial revolution and which had, in the same circles, been perhaps too hastily equated with the effect of increasing carbon dioxide as a trap for the outgoing terrestrial radiation. The argument that man-made dust is now more effective than the (still increasing) quantity of carbon dioxide has been drawn from the demonstrated effect of volcanic dust veils in the stratosphere in lowering the overall average temperature near the surface of the Earth by up to 1° C for a year or two after the great eruptions for which adequate observations exist.

But the analogy is largely false and, at best, of very limited value. Although even the greatest volcanic eruptions are estimated to put only from 1 million to 100 million tons of finely divided particles into the atmosphere, typical residence times in the stratosphere are several years. The mean residence time for particulate matter in the lower atmosphere is about 10 days, and it is then washed back to the Earth with the rain and snow. Moreover, solar radiation absorbed by matter suspended in the commonly soiled atmosphere in the lowest 1 to 3 km must be a more effective contribution to the energy budget at the Earth's surface than that which is intercepted by volcanic dust in the stratosphere. It does not take much thought to add to this list of *a priori* differences.

There is a great need for concerted efforts to establish the nature and magnitude of the meteorological and climatic effects of dust and other types of pollution in the lower atmosphere. Much survey work will be needed and many types of new observational techniques may be useful in supplying quantitative evidence of the facts.—

H. H. L.

Consequences and Truth

THE scientist feels that there is order in nature, if only it can be found. Indeed, such feelings have been the basis for most of scientific research, and results from all disciplines support this view. Thus the scientist is always looking for correlations.

Earth scientists are no exception, but they work in a field where proof, in the mathematician's sense of the word, is hard to find, or may be impossible to find because of the problems of observing geological processes at work. Earth scientists are normally presented with the results of

a process that may have lasted tens or hundreds of millions of years. Their job is to try to untangle the usually complex nature of the process from incomplete and often apparently conflicting evidence. This may perhaps be looked on as the reverse of a chemist's function, which is to control carefully the course of an experiment to produce an unknown or at best merely predicted result.

The concept of plate tectonics is a fine example of a unifying theory on a global scale which attempts to account for many different features on and in the Earth. Another such hypothesis was the Flood. It is perhaps unfair to compare the two, but many eminent scientists have spent much time looking for evidence in support of both of them. Both, in their time, seem to satisfy the scientist's craving for order in nature. It should perhaps be made clear that there are now some rather sinuous drawbacks to the Flood theory; it is amazing how one unifying theory after another has gone the way of the world, but this should come as no surprise and should be ascribed to progress.

According to plate tectonics the lithosphere (the outer 200 km or so of the Earth) moves about as a number of fairly rigid plates. In some places, for example along the line of the Mid-Atlantic Ridge, the plates are separating, because of the addition of material from below into the crack. In other places lithospheric material is being forced under the adjacent plate; the Earth is probably not expanding. If this concept of moving plates is accepted then many consequences follow, and it is these that many Earth scientists are engaged in examining at the moment, although, of course, the consequences do not necessarily mean that the hypothesis is the truth (see Cobbing and Pitcher, *Nature Physical Science*, **240**, 51; 1972).

This is the bandwagon, a very good one in my opinion, which is on the move in geology, providing the most intense stimulus to new ideas that the subject has ever had. It is thus very fortunate that the new exhibition of basic geological processes at the Geological Museum in South Kensington has appeared now, and not ten years ago.

On page 338 of this issue of *Nature*, P. R. Vogt of the US Naval Oceanographic Office examines one of the facets of the plate tectonic hypothesis, that of the possible effects of synchronicity in mantle plumes. Mantle plumes are more or less linear currents of material brought up from below the lithosphere, and it is believed that they may be one of the driving forces in plate movement. Vogt has examined the products (lavas) of two such plumes, in the Hawaiian chain and in Iceland, in detail and suggests that there is good evidence for synchronous events not only in these two plumes but also in a dozen more. He then attempts to relate this to other phenomena on a global scale (sedimentation rates in the oceans, magnetic reversals and biological extinctions). The relation between mantle plume activity and the "elimination of numerous genera" is particularly interesting. Vogt rehabilitates Professor Preston Cloud's argument that because marine or brackish water organisms are sensitive to the chemistry of their environment, perhaps the demise on a world-wide scale of certain genera can be attributed to changes in the concentration of trace metals in sea water.

Earth scientists are thus in their usual geological dilemma. They can observe the consequences, but what is the truth?—A. C. D.

A Sheep in a Wolf's Coat

THERE is to date no convincing evidence that any human cancer is caused either by infection by an RNA tumour virus similar to the avian and murine leukaemia and sarcoma viruses or by the expression of an endogenous tumour virus genome inherited through the egg or sperm. The search for traces of human RNA cancer viruses, however, continues apace.

Groups, notably those led by Gallo and Todaro, are, for example, isolating RNA dependent DNA polymerases from human leukaemia cells and attempting to show that these enzymes are closely related to the reverse transcriptases to be found in primate C-type RNA tumour viruses, while Spiegelman and his colleagues are using nucleic acid hybridization techniques to probe for RNA sequences in the polysomes of human breast carcinoma cells and leukaemia cells which have homology with the genomic RNAs of mouse mammary tumour viruses and mouse leukaemia viruses. And, of course, RD 114 virus, which has the biochemical and biophysical properties of a typical C-type RNA tumour virus and which may be of human origin, continues to attract much attention.

These approaches to the problem of identifying putative human RNA tumour viruses have been tried for more than a year without dramatic success, and against this background the experiments reported in a recent issue of *Nature New Biology* (240, 122 and 124; 1972) by Závada and his colleagues make very refreshing reading. This Czech group has made the interesting observations that vesicular stomatitis virus (VSV), an enveloped RNA virus which is not oncogenic and has no taxonomic relationship to the RNA tumour viruses, can acquire an envelope the antigens of which are specified by an RNA tumour virus when cells are simultaneously infected with VSV and, for example, either avian myeloblastosis virus or murine leukaemia virus.

This phenomenon, called phenotypic mixing, which gives rise to so-called pseudotype virus particles, is familiar to RNA tumour virologists because it is a general property of RNA tumour viruses. When, for example, chick cells are simultaneously infected with an avian sar-

coma virus of one subgroup and an avian leukosis virus of a different subgroup, the progeny particles liberated by the infected cells include, as well as the parental viruses, viruses which have the genome of one parent and the envelope, and therefore subgroup specificity, of the second parent and *vice versa*. In other words, the antigenic components of the envelope of an RNA tumour virus need not be those which the genome of that virus particle specifies. But until Závada and his colleagues made their discovery such phenotypic mixing had been observed only between one RNA tumour virus and another and then only between related viruses. For example, although murine, feline and hamster viruses phenotypically mix, mixing between avian and mammalian RNA tumour viruses has not been reported.

The fact that VSV genomes can occur in pseudotype particles with avian myeloblastosis virus or murine leukaemia virus envelopes is an entirely unexpected example of phenotypic mixing and is presumably non-reciprocal. Furthermore, using a temperature sensitive mutant strain of VSV, the particles of which are considerably more thermolabile than mutant VSV-tumour virus pseudotypes, Závada's group can, by heating, eliminate mutant VSV particles when they occur mixed with VSV pseudotypes and this greatly simplifies the detection of such pseudotypes.

Závada and his colleagues now report in *Nature New Biology* how they intend to use this mutant strain of VSV to search for putative human RNA tumour viruses. They infected a cultivated line of human mammary carcinoma cells with the mutant VSV and detected thermostable virus particles with a VSV genome and an envelope the antigens of which do not cross react with anti-VSV antisera. These particles may be pseudotypes comprising the VSV genome in an envelope the antigens of which are specified by a human virus present in the carcinoma cells, but this is, of course, extremely difficult to prove because to date nothing is known about the envelope antigens of human RNA tumour viruses. Obviously the group hopes that VSV will phenotypically mix with any human RNA tumour viruses should they exist in human cancer cells, and no doubt more than one laboratory is experiencing a sudden demand for VSV and suitable human tumour cells to use as hosts.

Clearly the novelty of this approach has great appeal, but it is worth bearing in mind that to date Závada and his colleagues have only detected phenotypic mixing between VSV and replicating avian and murine viruses. They have not yet shown that VSV pseudotypes are liberated by avian or mouse non-producer cells which contain RNA tumour virus genomes that are only partially expressed;

Cyclic AMP and Neuroblastoma Cells

CYCLIC AMP, that ubiquitous and seemingly omnipotent molecule, has of late been accused of mediating an extraordinarily wide range of changes in cells, including changes in the morphology and growth properties of cells in culture. It has, for example, been observed that cyclic AMP, or to be more precise dibutyryl cyclic AMP which penetrates cell membranes, induces the extension of neurites by cultivated neuroblastoma cells and chick embryo ganglia in culture. And it has been speculated that cyclic AMP effects this particular phenomenon by inducing the polymerization of microtubules, presumptive cytoskeletal elements of neurites. Kirkland and Burton, who are obviously enamoured of this idea, report in next Wednesday's *Nature New Biology* (December 13) experiments

which can be interpreted as lending support to it.

When mouse neuroblastoma cells which have formed neurites at 37° C are chilled to 4–8° C the neurites retract and the microtubules they contained disappear, only to reappear when the cells are warmed again to 37° C. When, however, dibutyryl cyclic AMP and Ca²⁺ ions are present most of the neurites are stabilized and the neurites remain when the cells are chilled. Apparently, therefore, in the presence of Ca²⁺ dibutyryl cyclic AMP can stabilize neurites perhaps by stabilizing their microtubules or perhaps by changing in some way the cell surface so that withdrawal is prevented. Kirkland and Burton discuss two possible ways by which this stabilization might be achieved.

such cells do not release progeny tumour viruses. Before large amounts of time, effort and money are spent infecting human tumour cells with VSV it would be advisable to prove that, for example, VSV pseudotypes are produced when Cofal positive chick cells, which contain an endogenous avian leukosis virus genome, which contain both group and subgroup (envelope) leukosis virus antigens but which do not release leukosis virus particles, are infected with VSV. Such non-producer cells are a more appropriate model of human tumour cells, which also do not liberate detectable amounts of C-type viruses, than cells supporting the complete replication of avian myeloblastosis virus or murine leukaemia virus.—From our Cell Biology Correspondent.

RECEPTORS

Applied Affinity

from our Molecular Biology Correspondent A. E. HOUSMAN once began a book review: "There is much in this book that is new and much that is true; unfortunately nothing that is new is true, and nothing that is true is new". The burden of a clutch of papers just out—simultaneously and with evidence of haste, for they are all in quick-fire journals—on the isolation of the acetylcholine receptor by affinity chromatography, falls into the true though not new category, which especially in a tricky and competitive field is the better alternative. The wide measure of agreement between the results of the three independent groups, and those of other workers, earlier reported (and touched on a few weeks back in this column), is distinctly reassuring.

The acetylcholine receptor has been extracted from the electric eel by one group, and from the electric fish, *Torpedo*, by two others. Olsen, Meunier and Changeux (*FEBS Lett.*, **28**, 96; 1972) have made an affinity column by coupling an acetylcholine analogue to 'Sepharose' beads, derivatized to carry reactive thiol groups. The tissue extract in non-ionic detergent is applied to a column of this material, and the bulk of the total protein washes through. The receptor activity and acetylcholinesterase are both retained, however, and the former can be collected by elution with a competing ligand, flaxedil. Concentrated salt serves to dissociate the adsorbed acetylcholinesterase from the matrix. The receptor can be assayed by its capacity to bind the radioactively labelled venom protein, α -bungarotoxin, the complex being retained on membrane filters. The up-

shot is that half the total receptor sites found in the tissue extract are recovered from the affinity column. The resulting purification factor is about 150, which is not, however, sufficient to generate anything like a homogeneous preparation, as judged by electrophoretic analysis in SDS-gels.

A rather higher purification is reported with the *Torpedo* receptor, caught on a column of cobrotoxin coupled to 'Sepharose' (Karlsson, Heilbronn and Widlund, *ibid.*, 107). After extraneous proteins have been eluted, the receptor is dissociated from the toxin by a gradient of carbamylcholine. The striking feature of the elution curve is that there are two peaks, both of which have receptor activity, binding tubocurarine with high efficiency. There is no acetylcholinesterase activity to speak of. The second component has the greater affinity for tubocurarine, but neither is altogether pure, for there is evidence of a background of other proteinaceous material.

The combining weights with toxin correspond to one site per 300,000 and 140,000 g respectively of the two components. Gel filtration of these components, carrying labelled venom, provides an estimate of their molecular weights (assuming a more or less symmetrical molecule), and both come out

at about 140,000. This is most probably then the molecular weight of the receptor, and it agrees with earlier estimates. The first peak from the affinity column is assumed to contain an inactive protein species. Karlsson *et al.* claim a purification of a thousand-fold. Half of the toxin-bound material, however, fails to elute in the carbamylcholine medium, and remains stuck to the column. Karlsson *et al.* offer the attractive, if insecurely founded, suggestion that the receptor might have two subunits, which can be of either of two kinds, only one of which is capable of binding such analogues as carbamylcholine. A not unimportant technical point seems to be that the preparation fails if done in the cold, because, Karlsson *et al.* suggest, the association with the column-bound toxin is then weak.

The third, and to an extent complementary, description of an affinity-based isolation of receptor, again from *Torpedo*, comes from Schmidt and Raftery (*Biochem. Biophys. Res. Commun.*, **49**, 572; 1972). The approach is that of Olsen *et al.*, in that the column-bound ligand is not a protein toxin, but a cholinergic species. The retention is not strong, however, and a dilute salt solution is enough to desorb the trapped receptor activity. The corresponding purification, in terms of α -bungarotoxin

Synthesis of Mitochondrial RNA Polymerase

IN next Wednesday's *Nature New Biology* (December 13), Barath and Kuntzel report the latest instalment of their exemplary investigations of the RNA polymerase in mitochondria of *Neurospora crassa*. Their experiments lead them to the interesting conclusion that mitochondrial RNA polymerase is not only synthesized by cytoplasmic rather than by mitochondrial ribosomes but also that this protein is specified by a nuclear rather than a mitochondrial gene. As Kuntzel and Schäfer reported last year in *Nature New Biology*, the RNA polymerase in *Neurospora crassa* mitochondria is resistant to α amanitin but sensitive to rifampicin and is composed of polypeptide chains with a molecular weight of 64,000.

To decide whether this polymerase is specified by mitochondrial DNA or by cell DNA, Barath and Kuntzel measured the synthesis of mitochondrial RNA polymerase in cells exposed to ethidium bromide or chloramphenicol, drugs which selectively inhibit transcription and/or translation of mitochondrial DNA. They found that in the presence of these drugs, mitochondrial RNA polymerase is synthesized in large amounts, and accumulates in the cytoplasm of the cells. By contrast, in the presence of ethidium bromide,

biosynthesis of cytochrome is inhibited.

These facts indicate that whereas the cytochromes are products of transcription and translation of mitochondrial DNA, mitochondrial RNA polymerase is synthesized in the cell cytoplasm and is coded by a nuclear gene. Apparently inhibition of mitochondrial transcription and translation induces excessive production of mitochondrial RNA polymerase so that cells which have been exposed to ethidium bromide for about 20 h contain in their cytoplasm crystals of protein which have the same molecular weight and the same electrophoretic mobility as RNA polymerase isolated from mitochondria.

This induction of excessive production of mitochondrial RNA polymerase by ethidium bromide leads Barath and Kuntzel to propose a model in which a repressor-like mitochondrial gene product binds to nuclear DNA and restricts synthesis of mitochondrial RNA polymerase. They suggest that when the synthesis of this mitochondrial gene product—the repressor—is blocked, the nuclear genes coding mitochondrial RNA polymerase are continuously expressed so that a large excess of mitochondrial RNA polymerase accumulates in the cell cytoplasm.

binding capacity, is also relatively modest. Gel electrophoresis in SDS gives a wide choice of protein zones, but a peak of 28,000 molecular weight is prominent and others are fairly clearly in evidence at 45,000 and 38,000. Schmidt and Rafferty proclaim their confidence, nevertheless, that the first of these is the genuine article, and in spite of the small subunit weight that this implies, take their courage in both hands and commit themselves to the assertion that previous estimates, for both *Torpedo* and electric eel receptor, of 40–50,000 are incorrect.

It might perhaps be remarked that, although the isolation of the pure receptor is clearly altogether a good aim, many of its properties, particularly, as Franklin and Potter (*FEBS Lett.*, **28**, 101; 1972) have shown, its interaction with such ligands as bungarotoxin, can be usefully studied on the homogenized membrane particles.

Regarding the question of molecular weight and subunit structure, it may be noted that a different membrane-bound receptor—that for adrenergic molecules in heart—has been purified by affinity chromatography on columns containing a bound norepinephrine derivative (Lefkowitz, Haber and O'Hara, *Proc. US Nat. Acad. Sci.*, **69**, 2828; 1972), and comes in two sizes, peaks corresponding to about 40,000 and 160,000 molecular weight being eluted. The simplest explanation is that different states of aggregation of the receptor protein coexist in the membrane.

PALYNOLOGY

Making Pollen Diagrams

from our Plant Ecology Correspondent

AN article by A. D. Gordon and H. J. B. Birks in the current issue of *New Phytologist* (**71**, 961; 1972) provides new hope for the introduction of objective techniques into the interpretation of data obtained by pollen analysis of Quaternary deposits.

There are many statistical problems inherent in the presentation and interpretation of pollen data and only one of these is tackled here, namely the construction of an objectively defined zonation system for a pollen diagram. Before this can be done one must assume that the original data are an adequate representation of the pollen content of the sediment undergoing analysis and that the data are basically reliable. One must also assume that the sedimentary sequence of a deposit reflects its temporal development; in other words, that the stratified sediments have not been disturbed subsequent to their deposition.

For several decades it has been the practice of palynologists to divide their

data into subsidiary groups, or zones, to facilitate the task of describing temporal changes in pollen proportions and the vegetational developments which these changes reflect. The use of zonation systems has been long established (for example, Godwin, *New Phytol.*, **39**, 370; 1940), but only recently have there been attempts to define the criteria which should be used for the subdivision of pollen diagrams (for example, Birks, *J. Ecol.*, **58**, 827; 1970).

The problem of objective zonation has been approached by using computerized classificatory techniques similar to those applied to vegetational analysis (for example, Dale and Walker, *Pollen Spores*, **12**, 21; 1970), but these have usually been of limited value because of the absence of any stratigraphical constraint written into the programme. This has allowed the possibility of grouping samples together from different parts of the profile on the basis of their similar pollen spectra. Gordon and Birks have overcome this problem by requiring that samples grouped together in a zone should be stratigraphically contiguous.

They use a variety of techniques, both divisive and agglomerative, to construct objective zonation schemes for published data from Britain and the United States. The zones resulting from these analyses are in remarkable agreement with the subjective decisions of the original authors regardless of the precise numerical technique used. This study, in addition to the ratification which it provides for the intuitive divisions produced by earlier workers, also offers a means of objective zonation for future work.

There are some major disadvantages in this type of approach; first, the selection by the computer of abrupt rather than gradual changes as the basis for zone boundaries; second, the tendency

to select zones defined by local rather than regional pollen types; and, third, the inability of the computer to place additional emphasis upon changes in species of critical ecological importance (for example, elm and plantain). In spite of these residual problems, Gordon and Birks's study represents an important step towards the removal of palynology from the realms of mysticism.

NUCLEAR PHYSICS

Ambiguity Resolved

from a Correspondent

ANOTHER way of removing the so-called "discrete ambiguities" of nuclear physics has been discovered. It involves the scattering of α particles from aligned nuclei.

Elastic α particle scattering can be very well described using the optical model of the interaction. According to this model, the α particle moves in a complex potential with radius and surface diffuseness similar to that of the nucleus itself, and with appropriate choice of parameters the differential cross-section can be fitted to high accuracy. Unfortunately, it is frequently found that there are several different sets of potentials which give equally good fits to the data ("discrete" ambiguities), and within each set it is often possible to make correlated variations of the parameters that preserve the fit ("continuous" ambiguities). Physically this is easily understood; elastic scattering depends only on the asymptotic form of the wavefunction and in addition the α particles interact so strongly with the nuclear field that those that are elastically scattered can only have passed through the surface region, so that all potentials giving the correct behaviour in this surface region give the same scattering.

Separating Myosin Functions

IN next Wednesday's *Nature New Biology* (December 13), Silverman, Eisenberg and Kielley describe a study of the consequences of blocking one thiol group (SH_1) in the heavy chains of myosin. It was previously reported that these groups could be reacted with N-ethylmaleimide, and the resulting protein was inhibited in regard to EDTA-ATPase activity, whereas the calcium-ATPase was enhanced.

Working with the more tractable molecule, heavy meromyosin (HMM), Silverman *et al.* have found that SH_1 -blocked protein is activated by actin, but by a factor of only three, compared with 200 for unmodified HMM. From the dependence of this small activation

on the actin concentration, the authors exclude any likelihood that the activation effect arises from a contaminant of unmodified HMM.

Silverman *et al.* have also investigated the effect of the relaxing system, troponin and tropomyosin, on the activity of the modified acto-HMM complex. In the absence of calcium the activation of ATPase by actin is completely suppressed, just as in normal actomyosin. It is concluded that the SH_1 thiol group is required not only for EDTA-ATPase activity, but also for activation of magnesium ATPase by actin, whereas it is not involved in the interaction with the troponin-tropomyosin system.

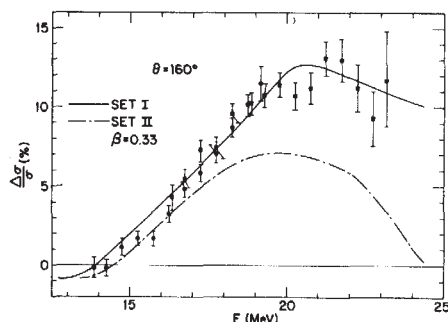
These ambiguities lead to difficulties when the potentials are used to generate wavefunctions for use in reaction calculations, so many attempts have been made to resolve them. In the case of deuterons, the low binding energy makes it easy to calculate the deuteron-nucleus potential from the constituent neutron-nucleus and proton-nucleus optical potentials, but for the more tightly bound helions, tritons and α particles this cannot yet be done with sufficient accuracy. The discrete ambiguities tend to disappear at higher energies, and this can be understood physically (see *Nature Physical Science*, **239**, 66; 1972).

Parks *et al.* (*Phys. Rev. Lett.*, **29**, 1264; 1972) have found another means of getting round the problem. Scattering from aligned nuclei is much more sensitive to the potential than the scattering from non-aligned nuclei—providing, of course, that the nucleus itself, and hence the potential, is deformed—and in their experiment, nuclei of the strongly-deformed ^{165}Ho were aligned by reducing the temperature to within a fraction of a degree of absolute zero. At these temperatures the forces within the metallic crystal align the nuclei, and this alignment is not destroyed by thermal motion.

The measurements were made by comparing the cross-section at 0.26 K, when the nuclei were aligned, with that at 1.75 K when they were not. Then the quantity

$$\frac{\Delta\sigma}{\sigma} \equiv \frac{\sigma(0.26 \text{ K}) - \sigma(1.75 \text{ K})}{\sigma(1.75 \text{ K})} = \frac{\sum_{m>0} A_m \sigma_m(\theta)}{\sigma}$$

gives a measure of the effects of the alignment on the scattering. This is shown in the diagram for incident α particle energies from 20 to 24 MeV for scattering through 160° . It is also expressible as a sum over the cross-sections $\sigma_m(\theta)$ for scattering by the nucleus in a magnetic substate m . The coefficients A_m can be calculated from the temperature of the crystal, its structure and the parameters of the nuclear hyperfine Hamiltonian, and $\sigma_m(\theta)$ can be obtained from a coupled-channels calculation of the interaction.



$\Delta\sigma/\sigma$ as a function of incident α particle energy. The two curves are calculated from optical potentials that give equally good fits to the differential cross-sections.

Thus $\Delta\sigma/\sigma$ can be calculated from the optical potential, and results of two such calculations, using potentials found by Aponick *et al.* (*Nucl. Phys.*, **A159**, 367; 1970) to give equally good fits to the differential cross-section, are also shown in the figure.

It is clear that one is consistent with the data and the other is not, so that the discrete ambiguity is resolved. The best value of the nuclear deformation found by optimizing the fit to the data ($\beta = 0.320 \pm 0.020$) agrees well with the results found by analyses of Coulomb excitation ($\beta = 0.33$) and photoneutron cross-sections ($\beta = 0.319 \pm 0.003$). The sign of β is determined because $\Delta\sigma/\sigma$ would have opposite sign for an oblate deformation.

ELEMENTARY PARTICLES

Muon Ranges Differ

MEASUREMENTS on positive and negative muons made at the Lawrence Berkeley Laboratory of the University

of California show that negative muons with a momentum of a few hundred MeV can travel about 0.2 per cent further in steel than can their positive counterparts of the same momentum. The experiment is reported by Clark *et al.* (*Phys. Lett.*, **41B**, 229; 1972) and can be explained in terms of a small correction to the familiar Bethe-Bloch formula for the rate of loss of energy of a singly charged particle.

Although measurements of range have long been used to estimate momenta, or, given separate measurements of momenta (by means of the magnetic deflexion method, for example), to distinguish particles of differing mass, it is only recently that accuracies of the order of fractions of a per cent in range measurements have become critical to the outcome of some CP violation experiments. And in the past cosmic ray physicists have usually been content with much larger accuracies.

Clark *et al.* used an experimental arrangement set up to look for the rare decay mode $K_L^0 \rightarrow \mu^+ + \mu^-$. With this

Two New Linkage Groups in Man

GENETIC analysis of somatic cell hybrids provides an increasing return as more data accumulate. In next Wednesday's *Nature New Biology* (December 13) van Someren, Mera Khan, Westerveld and Bootsma describe two new linkage groups in man. The information presented extends their previously published analysis of hybrids derived from fusions between Chinese hamster and human cells.

The novel contribution of the report concerns the relationship of the loci controlling, or coding for, glutamate pyruvate transaminase (GPT) and lactate dehydrogenase (LDH). GPT in red cells is polymorphic; two common and at least four rare alleles determined by an autosomal locus have been reported by Shi-Han Chen *et al.* Van Someren *et al.*, however, conclude from their observations on this enzyme in human fibroblasts and leucocytes that two additional loci exist which determine interacting subunits referred to as GPT-B and GPT-C (to distinguish them from the red cell form GPT-A). Electrophoretic analysis of white cell extracts revealed five bands of GPT activity clearly different from the red cell enzyme. The five-banded appearance is suggestive of a tetrameric structure and this, together with the genetic evidence implicating two loci, indicates a close parallel to the lactate dehydrogenase system. The subunits of the latter enzyme, LDH-A and LDH-B, can either interact to give a tetramer comprising combinations of two subunit types or form exclusive associates

of only one type. The GPT forms determined by the loci *gptB* and *gptC* can apparently associate in an analogous fashion.

Analysis of the electrophoretic patterns for LDH and GPT in extracts of the hybrid clones indicates that the loci for GPT-C and LDH-A and those for GPT-B and LDH-B are most probably linked. (About one-third of the sixty-eight clones analysed retained detectable GPT-B and LDH-B, the remainder having lost both. A similar proportion were positive for GPT-C and LDH-A. No disruption was observed for either linkage group.) Ruddle (*Adv. Hum. Genet.*, **4**, 173; 1972) has assigned the loci for LDH-A and LDH-B to chromosomes 11 and 12 respectively. Thus the data support the localization of the different LDH and GPT loci on two chromosomes which are morphologically similar.

Van Someren *et al.* suggest that these observations support the theory that gene duplication (in this instance resulting from chromosome duplication) may have given rise to enzyme subunits which although electrophoretically distinguishable are functionally similar and able to cooperate. Further analysis of markers on these and other chromosomes is obviously required if this suggestion can be considered more than speculation. Detailed genetic analysis of the type reported, however, represents one of the most direct ways to evaluate the possible involvement of chromosome duplication in the evolution of the human karyotype.

apparatus they recorded the fairly frequent decays of the form $K_L^0 \rightarrow \mu^\pm + \pi^\pm + \nu$. Muons from this decay passed through the magnet in one of the two arms of the apparatus and their range was measured in a stack of steel plates interleaved with scintillators. The possibility of systematic error was eliminated by reversing the fields in the magnets so that particles of both signs were recorded in each arm of the apparatus.

The distribution of the momenta of muons of each sign stopping at any point in either scintillator/plate stack could thus be built up and the difference in the mean momenta for each sign calculated.

After the necessary corrections had been made, the weighted mean of the fractional difference in range turned out to be $(2.28 \pm 0.20) \times 10^{-3}$ at the average momentum of 800 MeV/c.

CRYSTALLOGRAPHY

What Lies Ahead?

from a Correspondent

CRYSTALLOGRAPHERS of the future will have to broaden their horizons both to be able to gain support for their research and to obtain jobs in industry. This was a recurring theme in the conference on the role, support and organization of crystallography in the 1970s, arranged by the Crystallography Group of the Institute of Physics under the auspices of the United Kingdom Crystallographic Council and held in London on November 24. The point was made most forcibly by Professor R. Mason (University of Sussex), who until recently was chairman of the Chemistry Committee of the Science Research Council. He told the conference that support for research projects by the SRC will in future depend a great deal on the impact that the project is likely to have on the main stream of chemistry or physics.

Traditionally, the chief application of crystallography in chemistry has been single crystal structure analysis, but techniques have now developed to such an extent that structure-solving is in many cases a routine matter. To be worthy of SRC support such projects must emphasize the implications of the crystallographic results, for example, in new theories of chemical bonding. The same point was taken up by Professor J. D. Dunitz (Eidgenössische Technische Hochschule, Zürich) who said that although some crystallographers regret the fact that their principal art is becoming a routine, they should be glad that non-specialists can now solve crystal structures, leaving crystallographers free to make more sophisticated studies of crystals. Such studies

might include the investigation of intramolecular and intermolecular forces and their application in predicting crystal structures, and the extension of crystallographic ideas of close packing and linking of atoms into polyhedra to the study of liquids and solutions.

Many speakers emphasized the fact that crystallography is already a broad interdisciplinary subject. Dr M. F. Perutz (MRC, Cambridge) spoke about the important part that crystallography is playing in biology and medicine. The determination of the structure of DNA has led to an understanding of the genetic code and the mechanism of protein synthesis. Much progress has recently been made towards elucidating the structure of viruses and in understanding in molecular terms biological processes such as the aggregation of haemoglobin molecules in sickle cell anaemia. The whole intellectual background of biology and medicine has been transformed by the fact that these subjects can now be studied at an atomic level.

Professor J. Zussman (University of Manchester) discussed the role of crystallography in the earth and materials sciences. In these areas crystallography has moved away from being predominantly concerned with determinations of structure and is now also used to study texture and disorder and changes taking place during thermal transformations. Neutron diffraction and elec-

tron microscopy as well as X-ray diffraction are making important contributions in these fields.

A similar but more extreme dissociation of crystallography from structure determination was reported by Dr P. G. Owston (Polytechnic of North London) as having taken place in industry. His remarks were based on a survey he had carried out on the present and future industrial use of crystallography. Very little single crystal research is carried out, and the emphasis is chiefly on powder diffraction and electron microscopy. The materials studied seem to be principally inorganic substances, metals and polymers. The contrast between this industrial scene and university research, which concentrates on the determination of organic crystal structures, may help to explain the criticism sometimes expressed by industrialists that crystallographic training in universities is not as relevant as it should be. The most discouraging feature is that few industrialists can foresee any expansion of the applications of crystallography, a view that has serious implications for the future employment of crystallographers.

The conference concluded with a lively and sometimes controversial discussion initiated by a panel of young crystallographers. This not only recognized the need, but also expressed the readiness, to take a broader view of the subject.

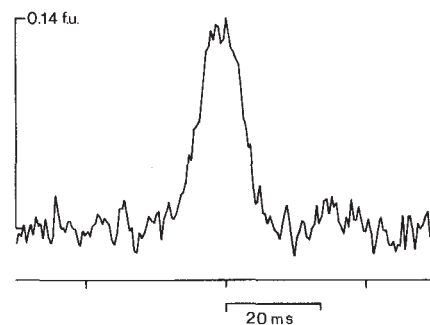
Six Pulsars Observed at 2.8 cm

THE new Max Planck Institute 100-m radio telescope has been successfully modified to operate at wavelengths down to 2.8 cm. In next Monday's *Nature Physical Science* (December 11), Wielebinski and colleagues report the first observations made by the instrument at this short wavelength; they are also the first observations of pulsars at 2.8 cm.

The telescope is designed on the homology principle (see *Nature*, **228**, 507; 1968), which requires continuous adjustments to allow for elastic de-

formations of the dish. It was not expected that the telescope would work as efficiently at 2.8 cm as at, say, 11 cm; but tests proved so successful that the Max Planck team immediately decided to concentrate first on pulsar studies where the large surface area is most advantageous.

Six pulsars were observed at 2.8 cm: CP 0329, AP 0823, CP 1133, PSR 1929, BP 2020 and JP 2021. Measurements carried out between October 21 and October 23 this year show that the steep spectrum characteristic of pulsars at longer wavelengths extends to 2.8 cm; above 1,420 MHz CP 1133 has the steepest spectrum and JP 2021 the flattest of the six so far studied. PSR 1929 is still highly linearly polarized at these high frequencies, and on the whole the pulse shapes of these pulsars are much the same at high frequencies as at lower ones (see diagram). Now that the 100-m instrument has proved effective at 2.8 cm, a series of studies at that wavelength, 6 cm and 11 cm will be made to provide more complete spectra on which more detailed models of pulsars can be built.



Pulse shape of pulsar JP2021 at 2.8 cm.

NUCLEATION

Clusters and Yet More Clusters

from our Molecular Physics Correspondent

ALTHOUGH some research fields seem to grow from a single, germinal experiment, others take root in accidents set off by workers stumbling from one preserve to another—and are none-the-less interesting for that. One might almost argue that a topic generated by coalescence of different interests is more ripe for progress than other more spectacular, though isolated, discoveries.

Nobody will pretend that the recent, intense activity centred on the detection and manipulation of small atomic and molecular clusters is likely to produce a very large salient at the frontiers of science—all the same it would be hard to find a neater and more stimulating set of experiments than those which have initiated this many-sided and completely original aspect of experimental rare-gas dynamics.

Three quite independent groups seem to share the honours in the crucial step, the discrimination and measurement of simple Van der Waals aggregates of 2, 3, 4 up to some 100 atoms. Priority goes to Becker, Bier and Henkes (*Z. Physik.*, **146**, 333; 1956) who noted the increase in intensity of nozzle beams due to cluster formation and performed the first mass spectrometric analysis of the products. At that stage, although the onset of nucleation in nozzle expansion was well known through the classic experiments of Oswatitsch in the 1940s, nobody could be sure whether the measured cluster equilibria were established after the mass spectrometric ionization stage or were a truly thermodynamic effect. Two other laboratories contributed to the removal of these doubts, entering the subject with, it seems, a totally different viewpoint and objectives. Greene and Milne (*J. Chem. Phys.*, **39**, 3150; 1963) examined argon clusters from similar beams and were able to detect and mass-analyse nuclei as small as the dimer, while at about the same time Leckenby, Robbins and Trevalion (*Proc. Roy. Soc., A*, **280**, 409; 1964) found clusters in the vapour of alkali metals, a system abounding in both practical and theoretical importance, from a conventional vapour effusion source.

The different points of departure seem worth recalling—Leckenby, Robbins and colleagues seem to have been involved with reactor engineering aspects of liquid sodium; Milne and Greene give their original interest as techniques of ion extraction from flames; Becker, Bier, Henkes and their collaborators give quite the most unexpected motive of the three—the eventual production of charged deuterium

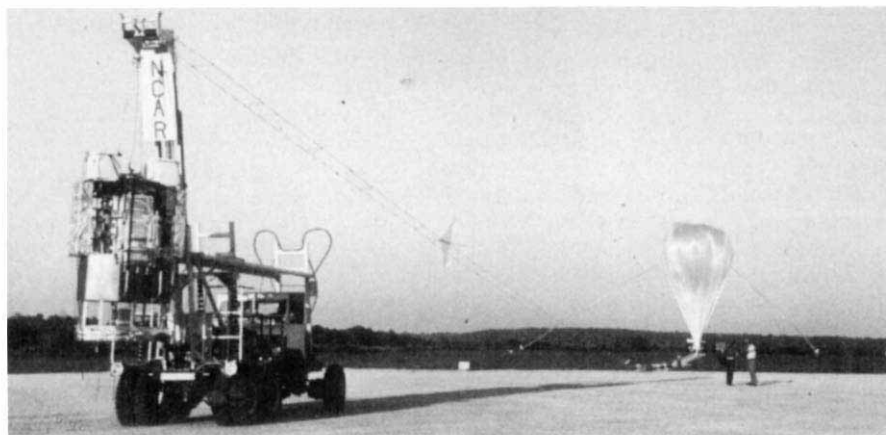
cluster ions for acceleration into thermonuclear machines. It seems a pity that little more has been heard of this project.

With the general techniques for obtaining observable concentrations of clusters now well established, more specialized attention has recently been devoted to the detailed explanation of the nozzle thermodynamics (Hagena and Obert, *J. Chem. Phys.*, **56**, 1793; 1972), to the kinetics of cluster growth and decomposition (Golomb, Good, Bailey, Busby and Dawbarn; *J. Chem. Phys.*, **57**, 3844; 1972) and to the measurement of special properties such as the ionization potentials of metallic systems (Foster, Leckenby and Robbins, *J. Phys., B*, **2**, 478; 1969). Around all this a healthy body of theory has begun to grow, an idea of which might be given here by some representative ques-

tions, all of them the idlest of speculation ten to fifteen years ago, but now well on the way to being experimentally decidable.

Is there a corresponding-states principle for different gases undergoing nozzle condensation or a related "corresponding jets" principle in the dynamics? (The answer is in the affirmative according to Hagena and Obert, with a possible reservation if quantum effects are involved.) Does the equilibrium concentration of dimers give more, or precisely as little, information about the interatomic potential as good second virial coefficient data? (More, insist Milne and Greene (*J. Chem. Phys.*, **51**, 1685; 1969), replying to criticism with the point that the mass spectrometer samples stable clusters while the virial coefficients reflect averages over both these and metastable ones at higher energy.) How quickly does the ionization potential of, say, Na_n clusters fall to the work function of the bulk metal as n increases? (It is about half-way

Ultraviolet Observations of Interstellar Mg



A BALLOON-BORNE objective grating spectrograph has been used to observe interstellar lines of MgII and MgI in the spectra of six stars. In next Monday's *Nature Physical Science* (December 11) Boksenberg and his colleagues describe the results of this joint experiment carried out under the auspices of University College, London, and Queen's University, Belfast, which has led to estimates of the interstellar electron density.

The balloon (see figure) was launched from Palestine, Texas, on October 4, 1972, and floated at an altitude between 42 km and 40 km for 12 h; at these altitudes, zenith atmospheric transmission in the relevant part of the ultraviolet (Mg lines are seen around 2800 Å) is about 50 per cent. Five of the stars observed were in Orion and the other in Cassiopeia; in general, the MgII lines

were strong and the derived column densities of Mg^+ did not depend crucially on velocity models of the distribution in the line of sight. The cosmic abundance of Mg relative to H is 2.8×10^{-5} , and the values of magnesium abundance found for γCas and γOri are close to that value. But for the three stars of Orion's belt, the abundances are rather higher.

The average value of the electron density for cool clouds in the line of sight to these five stars is $(1.5 \pm 0.6) \times 10^{-3} \text{ cm}^{-3}$, assuming a cloud temperature of 60 K, according to the calculations of Boksenberg *et al.*; because the derived values of this parameter are similar for all the observed stars they suggest that the best interpretation is that most interstellar absorption occurs in foreground cool clouds rather than in the vicinity of the stars.

there by $n=4$, according to Foster, Leckenby and Robbins.) Given the surprisingly high concentration of dimers under supersaturation conditions, might not the standard growth mechanism for nucleation: $A_n + A \rightarrow A_{n+1}$ be replaced by the alternative process $A_n + A_2 \rightarrow A_{n+1} + A$, a possibility thought long ago to be discredited? (Further results bearing on this are urgently needed.)

If a trend can be discerned in these varied interests it is probably towards more precise investigation of the smallest aggregates, particularly the dimer and trimer, whose formation, under some conditions, may well represent the rate-determining step in a total condensation process. The existence of rare gas dimers has long excited the curiosity of spectroscopists and, appropriately enough, there has been renewed activity in this direction in recent months both in theory (Levine, *J. Chem. Phys.*, **56**, 2455; 1972) and experimental detection (Tanaka and Yoshino, *J. Chem. Phys.*, **53**, 1012; 1970).

For theoreticians these results provide almost an *embarras de richesse* in a field where for so long they have been forced to work against a background of *ad hoc* assumptions varying from the feeble to the outrageous. Now, most recently, they have received a further gift which, in the context of what went before, can be seen as nothing short of a godsend. After some inconclusive earlier attempts, a group at the Laboratoire de Diffraction Electronique at Orsay (Raoult, Farges and Torchet, *Acta Cryst. Suppl.* (International Congress of Crystallography, Kyoto, 1972)) have succeeded in crossing an electron beam with a condensing nozzle beam and obtaining diffraction patterns of sufficient accuracy for a quantitative elucidation of the type of clusters present. The sharply structured scattering curves leave no doubt that the aggregates concerned in the range 100 to 500 atoms at least are essentially microcrystalline in nature rather than glassy or liquid microdrops. Similar determinations should be possible with even smaller systems and will be awaited with great interest, for present speculative opinion is that, if there is a size region in which a microcrystallite must "make up its mind" to take on a definite lattice with long-range order, it should be around some 50 to 100 atoms.

The importance of the diffraction studies lies not only in the encouragement they will give to the use of solid-like statistical mechanical models—much easier to handle than liquid drop theories—but also in the fact that they provide a link for the first time between experiments in rare-gas condensation and the considerable body of work which has been assembled on the structure of microcrystalline metal particles

in thin films and smoke deposits. These may also be investigated by diffraction and micrography to a lower limit of some 100 atoms (see Komoda, *Jap. J. Appl. Phys.*, **7**, 27; 1968), although always in rather more intimate association with a supporting substrate than would be preferred. Indeed, if the new

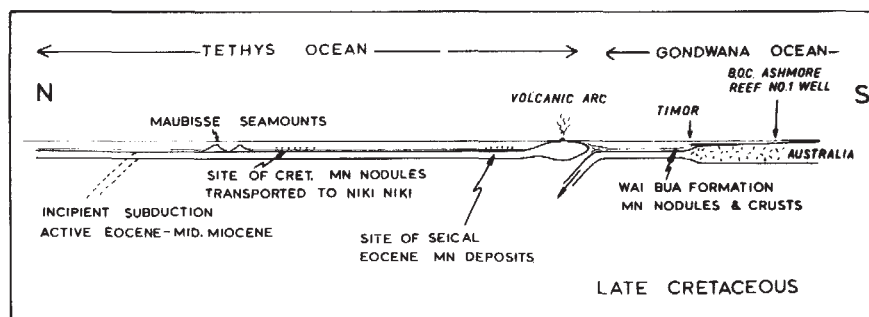
studies on clusters in free space were to achieve no more than the removal of doubts about interference with structures by the support material, the great pains taken in obtaining them would be well justified. In the event, far more exciting features than this seem likely to emerge in the future.

Manganese Nodules in Indonesia

A FEW months ago, Audley-Charles *et al.* (*Nature Physical Science*, **239**, 35; 1972) proposed the first detailed model for the plate tectonic evolution of Indonesia. The series of orogenic events recorded in the Outer Banda Arc and eastern Sulawesi since the breakup of Gondwanaland were viewed as the result of a series of collisions between the northward-drifting Australian continent and a series of northward-dipping subduction zones. The first collision (with the right-hand subduction zone in the accompanying figure) took place between the late Cretaceous and the Eocene, following which renewed subduction began further north (the left-hand, incipient, zone in the figure) to allow Australia to continue its northward drift. The new subduction zone was then overridden during the middle Miocene. A third subduction zone then developed even further north, with which Australia collided during the late Pliocene.

Timor during the first collision. If the former were true the nodules would no longer exist; if the latter had occurred they would be found below Timor's Palaeogene and early Neogene autochthonous deposits. By the same token, the nodules almost certainly originated south of the Maubisse Islands because the ocean floor to the north of these islands was consumed in the second subduction zone. Most of the ocean floor to the south of Maubisse must, however, also have been consumed before the second collision. Audley-Charles thus envisages that the nodules and their clay matrix must have been saved from consumption by the Maubisse Islands. The idea here is that the Maubisse Islands, some of which were clearly not consumed, acted as a scraper, preventing the consumption of the nodules even as the rest of the ocean floor to the south slid down the subduction zone beneath.

Finally, an odd feature of Timor is



In next Monday's *Nature Physical Science* (December 11) Audley-Charles uses this model to explain how fossil manganese nodules come to lie on the Indonesian island of Timor. The nodules of western Timor, which were described by Molengraaff (*Proc. Acad. Sci. Amst.*, **23**, 997; 1922) fifty years ago, resemble modern deep sea nodules, being combined with micronodules in a matrix of red clay which is similar to recent deep sea red clay.

But how did it come about that this late Cretaceous mixture now lies on land about 480 m above sea level? The mixture is now floating in a matrix of middle Miocene olistostrome. Audley-Charles thus argues that the nodules must originally have been deposited north of the first subduction zone; otherwise they would have been either consumed in that zone or swept onto

that the fossil deep sea manganese nodules of the west have, as close neighbours, Eocene ferromanganiferous deposits in the northeast (Seical Formation) and distinct Cretaceous ferromanganiferous crusts and nodules in the southeast (Wai Bua Formation). The evidence suggests that the Seical deposits were also formed on the deep ocean floor, but closer to the first subduction zone. But the Wai Bua nodules and crusts, though Cretaceous, seem not to be deep sea formations but continental margin deposits. Thus they presumably originated while Timor formed part of the northern margin of Australia.

In fact, Audley-Charles shows that three distinct manganese deposits formed in the three quite different regions shown in the figure now coexist on the same Indonesian island.

Assessing the Council for Scientific Policy

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This article examines the membership, operations and purposes of the Council for Scientific Policy (CSP) and offers some interim conclusions about the outcome of the CSP experiment. It was written while the CSP was still in existence.

Now that the Council for Scientific Policy is to be reconstituted¹ with a composition, though not a function, similar to that of its predecessor, the Advisory Council on Scientific Policy (ACSP), it is appropriate to make an assessment of the CSP's work.

Although sometimes mistaken for the secretariat of the Science Research Council (SRC), the CSP is in fact a body charged with advising the Secretary of State for Education and Science in the exercise of her responsibility for government science policy. Its origins can be traced to the post-war ACSP, and to the acceptance in 1963 by the Conservative government of the recommendations of the Trend Report², but the council only became effective in 1965, under the reorganization of government science by the newly elected Labour government.

The organizational context of the council's operations has changed during its lifetime, but within the Department of Education and Science (DES) the CSP has been responsible to the Secretary of State and to the Minister of State for Higher Education and Science. It advises on the allocation of the "science budget", which is chiefly accounted for by the budgets of the research councils and the Natural History Museum, and grants to the royal societies. The council was connected by overlapping membership with the Advisory Council on Technology of the Ministry of Technology from 1965–70, and also overlapped with the Central Advisory Council for Science and Technology, established in 1967 at cabinet level and now in abeyance after a short lifetime of indeterminate value.

Although it has these connexions with other aspects of the making of science policy, the responsibilities of the CSP have been confined to work financed by the DES, that is, principally the work of the research councils. The range of its oversight has therefore been shorter than that of its predecessor, the ACSP, which had also advised on, among other things, atomic energy research and the industrial functions of the former Department of Scientific and Industrial Research, both of which were transferred to the Ministry of Technology by the Labour government. On the other hand, whereas previously the research councils had negotiated their budgets individually with the Treasury and the ACSP had played no part in these proceedings, this situation was changed by the Science and Technology Act of 1965. Research councils were henceforward to be financed through the Secretary of State for Education and Science, who was to be advised in this matter by the CSP. Thus the nature of the advice sought from the CSP covered a smaller area than previously, but adopted a much greater financial orientation.

To gain an impression of the scale of finances involved, it may be noted that in 1967–68 the total national expenditure on research and development was almost £1,000 million, or 2.7% of the GNP. Rather more than half of this was provided by the government, and the rest came from private industry and public corporations³. Of the funds provided by the government, a little less than half financed defence research and development, the remainder being spent chiefly by the Ministry of Technology and the DES. Expenditure by the DES, with which the CSP is concerned, was £72.6 million in 1967–68; it was £53.6 million in 1965–66, and was later to rise to £117.2 million in 1971–72 (ref. 4).

Members of the CSP

The council, which first met in February 1965, was chaired initially by Sir Harrie Massey, then also Quain Professor of Physics at University College, London. He was succeeded in late 1969 by Dr (now Professor Sir) Frederick Dainton, then Vice-chancellor of the University of Nottingham and now Professor of Chemistry at Oxford.

The membership in those early days (including the chairman) consisted of fourteen scientists drawn largely from the universities, although two were from industry. There were also non-voting assessors from the four research councils and the University Grants Committee (UGC). The fourteen scientists were independent members—that is, they did not formally represent any particular groups within the science community. Although the individual members have changed somewhat since 1965, the overall membership pattern has remained broadly similar, the chief difference being the addition of members and an assessor to represent the social sciences. Other government departments have been represented when appropriate at the council's meetings.

Information has been collected about the background of CSP members, using as samples the membership lists for 1966 and 1967 (taken together), and for 1971 and 1972 (also taken together) (see Table 1). The samples included the chairmen, but not the assessors, who, as well as not being full members of the council, are appointed *ex officio* rather than in their own rights. The sample for 1966–67 consisted of sixteen men, and the sample for 1971–72 of nineteen men and one woman (a social scientist).

In 1966–67, fourteen out of the sixteen were fellows of the Royal Society when they joined the council, and a fifteenth became a fellow during his term with the CSP. Both samples show a sprinkling of Nobel Laureates, and more than a sprinkling of members of the Athenaeum Club. The typical CSP member is between forty-five and sixty years old, and only one man has been younger than forty-five on joining the council.

The education of CSP members has been noticeably centred on the University of Cambridge. Nine from the earlier sample, and twelve from the later one, had received either their first or second degree from that university. The next most popular universities, but a long way behind Cambridge, were London and Oxford, and the same three universities were the chief employers of members of the CSP. An analysis of the academic background of members shows an approximate equality of physical and life scientists, with engineers, social scientists, and

Table 1 Biographical Details of CSP Members in 1966-67 and 1971-72 *

	1966-67†	1971-72‡
Professional standing		
Fellows of the Royal Society	15	14
Nobel Laureates	3	3
Members of the Athenaeum	12	9
Age‡		
Less than 45	1	0
45 to 49	4	7
50 to 59	7	9
60 and above	4	3
Number of members who received either their first or second degree from§		
University of Cambridge	9	12
University of London	2	4
University of Oxford	1	2
Academic background		
Physical science	6	6
Life science	5	7
Engineering	3	3
Other natural science	2	2
Social science	0	2
Occupation at time of joining CSP		
Academic at London	5	3
Cambridge	2	3
Oxford	1	3
Edinburgh	2	0
Other universities¶	3	5
Total number of academics	13	14
Number of industrialists	2	3
Number in government service	1	1
Number in other employment	0	2

* The source was chiefly *Who's Who*, but with one or two details from other standard reference books.

† The 1966-67 sample totalled sixteen members, and that for 1971-72 twenty. Both samples included chairmen but excluded assessors. Details given in the table apply to the full samples except where otherwise stated.

‡ Age for the first sample taken on January 1, 1967, and for the second on January 1, 1972. The age of one member of the second sample has not been found.

§ Only those universities from which at least two members in a sample graduated have been listed. The relevant information has not been found for two members of the second sample.

|| "Other science" includes geology and biophysics, which do not readily fit the simple classification used.

¶ None of the "other universities" employed more than one CSP member.

other, unclassified, subjects (namely, geology and biophysics) being rather less well represented. The balance between physical and life scientists does not reflect the preponderance of physical over life scientists in the national scientific community.

It seems fair to infer from this information that the backgrounds of the members of the CSP have much in common. The differences between them are small compared with the variation across the whole British scientific community, especially in terms of professional standing, educational origins, age and occupation.

Members of the CSP are selected through a vaguely defined consultative process, at the end of which names are offered to the Secretary of State for approval or rejection, and it is not unknown, even after all the preliminary screening, for a minister to decide against a particular nominee. Part of the rationale of the process of selection seems to be that the council should comprise leading senior scientists from a fairly broad distribution of disciplines. One advantage of this is that the council is structured to reflect possible dissent between disciplines (for instance, about the level of support for nuclear physics). A disadvantage is that the council does not necessarily mirror disagreements between, say, younger and older research workers about the pattern of research within a single discipline. Consequently, the CSP is representative of the scientific community in only a restricted sense.

Operations of the Council

Meetings of the full council take place about twelve times a year. The agenda for these meetings are compiled by a preparatory group, which includes the chairman, deputy chairman, and two or three other members, and items for the agenda can be proposed by any member of the council. The assessors from the research councils and the UGC attend the meetings and play a full part in the proceedings. Their lack of voting powers is hardly significant in a body which can be expected to vote on only the rarest of occasions. The Secretary of State, Minister of State for Science, senior officials from the DES, and representatives from other departments also attend occasionally.

Much of the council's work is done not in full meetings but through committees, working groups and steering groups, of which more than twenty have been established since 1965, although not all of these still exist. There are two standing committees, one on international scientific relations, created in 1965, and the other on human resources in science and technology, established in November 1969. Among the reports which these groups have produced are those on computers for research, the flow of candidates in science and technology into higher education, liaison between universities and government research establishments, molecular biology, the implications of the proposed 300 GeV accelerator, and the support of scientific research in universities (for a full list see ref. 5).

The council has produced only three general reports about its activities⁶⁻⁸. The first of these produced an outcry at its advocacy of the need for forward planning so as to ameliorate the effects of an anticipated reduction in the rate of growth of the science budget. Through being in step with government thinking over the issue of economic planning, the CSP found itself in advance of the scientific community. The third report was promised for 1969 (ref. 9) but did not then appear, and its eventual publication was further delayed by the uncertainty about the council's future that followed the Rothschild proposals¹⁰.

The council is supported by a secretariat, headed by a civil servant with the rank of assistant secretary, and comprising (in 1969) a senior principal scientific officer, a principal, two principal scientific officers, an assistant principal, a higher executive officer, and support staff¹¹. Except for the occasional man seconded from another department, the secretariat consists of officials drawn from the DES. The task of acting as secretariat to the council is, therefore, only part of their duties, and they may, at least on occasion, be in the difficult position of simultaneously serving two masters in connexion with the same issue. In assisting the CSP they inevitably acquire a deep knowledge of opinion within the council on any issue, and as officials of the DES they may be required by the Head of the Science Branch of the DES, for example, to provide critiques of council documents. In this situation it would be unusual if dilemmas of discretion did not frequently arise and, while this is not atypical of practice in the civil service, the situation in this case may sometimes be undesirably sensitive.

Functions of the CSP

The CSP has described its purpose as two-fold: first, it must ensure that the activities within its purview "have sufficient resources for their proper development; and second, that within this total of resources the best value is obtained by distribution between the agencies concerned"¹². These responsibilities may be conceived analytically as corresponding to two functions, that of promoting science and that of regulating it. A similar functional division was implicit in a statement made in 1966 by the then deputy secretary in charge of the Science Branch (Sir Frank Turnbull), in which he said¹³ that "there has perhaps been some feeling that because we have four research councils, the interests of basic science are divided and have not been presented as a unity to the government in a

coordinated way. The main purpose of constituting the new Council for Scientific Policy is to meet this feeling. It is also to meet a need felt on the administrative and financial side, namely that it is very difficult to deal with four bodies, some of which have interests which are very near to one another".

Promotional Function

It was in its promotional capacity that the CSP published, in the 1967 Report on Science Policy, a lengthy general argument for the support of science. The argument was based on the premise that science is the "prime mover", supplying the new knowledge and leaders essential to the progress of society. This premise was clearly expressed in the well-known sentence: "Basic research provides most of the original discoveries and hypotheses from which all other progress flows."¹⁴

While admitting the difficulty of relating much basic research to any kind of benefits other than intellectual ones (except, perhaps, in the very long term) the council argued that basic research is the ultimate source of most technology, and that all areas of basic research are linked in an interdependent fashion. Thus science and technology were envisaged as a web of activities, the elimination of any part of which would weaken the whole structure. Consequently it was possible to argue that basic research is of economic as well as intellectual importance, because without it the impetus to innovate would be lost. Moreover, even such esoteric areas as nuclear physics could now be justified in economic terms, because a reduction of effort in such fields would weaken the whole fabric of science and hence of technology as well. That this was an oversimplified view (see ref. 15, for example) has since been accepted by the council¹⁶. Indeed, the CSP working group on economic benefits went so far as to conclude that "curiosity-oriented research is only rarely the mainspring of substantial innovation"¹⁷, thereby considerably amending the council's initial theory of innovation. This does not mean, of course, that the CSP and its working group now believe university research to have no economic value, but that they consider it more accurate to describe such research as "providing an infrastructure on the basis of which innovation or improvement in technology is possible"¹⁸, rather than as the prime source of technological development.

Although the second general report contained the clearest manifestation of the council's promotional activities, there are other examples. Many of the reports of the working groups, insofar as they involved making a case for expenditure on science, fall into this category; so, too, do the conferences organized by the council at Manchester in 1971 and at Strathclyde in 1972. The CSP has also fought hard for the preservation of the research councils during the recent Rothschild debate. Furthermore, these examples refer only to the public aspect of its promotional activities; doubtless still more effort has been expended by the CSP on behalf of the research councils within the closed confines of Whitehall.

Regulatory Function

The CSP is an advisory body with no executive powers over the research councils. The expression "regulatory function" is intended only to suggest that the CSP may influence the programmes of the councils by virtue of the method whereby it formulates its advice to the Secretary of State, and by virtue of its knowledge of the views of the Secretary of State on various issues. That the concept of regulation has substance in this context is supported by the council's statement that its advice on the budgets of the research councils "can influence directly the resources made available to the councils and, indirectly, their policies through making suggestions for the priority to be attached to research in a certain field or by indicating the need for closer coordination of policies in particular areas"¹⁹.

Before advising on the allocation of resources to the research councils, the CSP discusses with each council its budgetary

proposals, most of which will naturally consist of provision for existing commitments. That CSP approval for research proposals is extremely desirable, if not absolutely essential, was confirmed to some extent by Mrs Thatcher in evidence given to the Select Committee on Science and Technology in May 1971, when she said that although the research councils determine their own policies to "quite a considerable extent" they nevertheless have "to justify it to the CSP". The process is one in which the councils "come before a forum of a very wide range and group of scientists and argue for their own particular subjects"²⁰.

The CSP may also exercise a regulatory function through the transmission to the research councils of ministerial views about certain research areas. Officially, the research councils are autonomous and can spend their budgets as they choose, although technically they require departmental approval for large projects (see, for example, the evidence of the SRC to the Select Committee on Science and Technology²¹). The principle of autonomy has been insisted on time and again, and it has been vigorously denied that the Secretary of State has ever intervened directly in the affairs of the research councils²². Yet in at least one case, that of the CERN 300 GeV accelerator, it seems clear that intervention did take place, although its ultimate source may have been higher than the Secretary of State.

The CSP, after taking advice from the SRC and a CSP working group, approved the project, albeit without much conviction²³. The government, however, decided not to support the project—*prima facie* a direct intervention. Dainton himself did not wholly deny this in evidence to the Select Committee on Science and Technology²⁴, and the Minister of State for Science, Mrs Shirley Williams, spoke, at the time of the decision, of the need for the government to "step in" on this occasion²⁵.

Intervention along such direct lines has probably been rare—this may well have been the only example of its kind. The more usual style of ministerial involvement, to use the words of the current Permanent Under-secretary for Education and Science, Sir William Pile, would take the form of "showing an interest in certain activities and hoping and expressing the hope that some would be increased and that others might be slightly run down"²⁶. Given that there are occasions when the minister actively seeks to influence the programmes of the research councils, the CSP would seem to be one likely vehicle for such influence. Direct contact between the minister and the council at occasional council meetings, or indirect contact through civil servants in the DES, are two possible mechanisms for the transmission of ministerial views to the CSP.

It is suggested, then, that because of its knowledge of the plans of the research councils, and, in varying degrees over various issues, of the intentions of the Secretary of State, the CSP can act as a two-way intermediary between the research councils and the Secretary of State. It is from this position that its regulatory influence is derived.

Assessment

In any assessment of the work of the CSP it should first be noted that there is some reason to suppose that there was a shift in the division of effort between promotional and regulatory work from about 1968. The absence of the promised third full report, and the failure of the council to produce any reports from working groups from mid-1968 until 1971, suggest that it was more concerned about allocation of resources than about the public promotion of science. Dainton has partly confirmed this view, saying: "I think we could have produced more informative annual reports than we have done. The fact of the matter is that we have been rather hard-pressed over the last few years, having had a lot of difficult problems, partly concerned with the run-down of the growth rate—what you might call the regulatory function."²⁷ A further cause of the council's silence, he added, was its strenuous efforts to

develop criteria for choosing between scientific projects—"an almost intractable problem".

A second observation is that the council has been unlucky in its attempt to plan the growth of the science budget. While it was able to announce, in 1967, that provisional agreement had been reached on funds for up to three years ahead, saying that this gave "some substance to the concept of planning for science"²⁸, these good intentions were soon lost in a succession of national economic crises.

A third point is that the advisory status of the council has probably made its regulatory function more difficult to perform than it might have been. The disadvantages of the current system were summarized in the Dainton report on the future of the research council system, where it was stated that the council's "ability to determine policy has . . . been limited to the influence which its discussions and published reports may have . . . and to the consequences for the research councils of its advice on financial allocations"²⁹.

It was because it saw a need for both stronger coordination of the often overlapping interests of the research councils, and a greater involvement of government departments in the formulation of science policy, that the Dainton committee proposed the dissolution of the CSP and its replacement by a differently constituted and rather more powerful board of the research councils. A modification of this proposal, this time envisaging the board as a purely advisory body, was outlined by the CSP in evidence to the Select Committee on Science and Technology³⁰. This second suggestion from the CSP closely resembles the solution eventually adopted by the government, although in providing that the executive heads of the research councils and the UGC should be full members of the advisory board, the government was here also following Rothschild's proposals³¹. The board is to advise the Secretary of State not only on the allocation of the science budget of the DES, but also on the structure of the research council system, a responsibility which was specifically requested by the CSP.

A final comment is that the notion of regulation is related to that of accountability, and it is in connexion with this concept that the CSP has perhaps proved most disappointing. Although the research councils publish annual reports of their accounts and activities, they are not, as Lord Rothschild has pointed out, publicly accountable for the content of their research programmes³². The CSP is not only ideally placed to exert programme control over the research councils, but in writing, as it did, of its function of obtaining the "best value" for the money available, it may be considered to have shouldered the task of exerting such control as best it could with the limited powers available.

Yet, in the judgment of Sir Brian Flowers, what the council has actually done is to repeat the work of the research councils by assessing the quality of research programmes (admittedly across a very broad front) rather than laying down the general framework of priorities within which the councils should be constrained to operate³³. Its approach has been characterized instead by the attempt to maintain a "balance" between research areas. This is the normal style of the DES, as shown by Sir Frank Turnbull's observation in 1966 that "as regards priorities, perhaps I should say quite briefly that we do not think in this sort of way. What one needs to do, I think, is to have a reasonably balanced programme for carrying out the objectives for which one is doing research"³⁴.

If the CSP has not laid down a general order of priorities for the programmes of the research councils, it has at least responded to mounting financial pressure and to demands for a social return on investment in science. A working group on the determination of scientific priorities was created in March 1970 under the chairmanship of Professor F. H. Stewart and has sought to make explicit the criteria used in taking decisions about research support. It nevertheless admits that an important element of subjective judgment is necessarily required in weighting the criteria in individual cases³⁵. This, of course, should occasion no surprise.

The inherent difficulty of determining priorities is not, however, a sufficient argument against making the attempt. It is also difficult to assess the current state of interdisciplinary balance, yet such assessments have perforce had to be made, and the mixture of criteria, tacit knowledge, and informed personal perceptions used for this purpose could presumably be transferred to the determination of priorities. For, while inflexible adherence to a fixed set of priorities is clearly undesirable in the context of rapid change, nevertheless such a set, if regularly adjusted, could help significantly in holding the research councils accountable, in advance, for the content of their programmes.

There is some reason to suppose that the new advisory board will be better equipped than the CSP for the task of determining priorities. It is to receive information from customer departments about work which they have contracted out to the research councils³⁶, and this will almost inevitably involve the board in discussions about the relationship between such work and the other research supported by the departments. This information, coupled with the greater departmental representation on the new board, may well enable it to take a broader view than the CSP ever could of the essential pattern and objectives of the national research effort. If this proves to be so, it will encourage the new board to consider the programmes of the research councils more specifically in terms of overall national objectives than perhaps was ever possible for the CSP. In that event, enhanced accountability, in the most fundamental sense of that term, must inevitably ensue in the research council system.

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- ¹ *Framework for Government Research and Development*, para. 45, Cmnd 5046 (HMSO, July 1972).
- ² *Report of Committee of Enquiry into the Organisation of Civil Science*, Cmnd 2171 (HMSO, 1963).
- ³ *Statistics of Science and Technology, 1970*, 3 and 8 (HMSO, 1970).
- ⁴ *Select Committee on Science and Technology, Report on Research Councils*, HC 522, Table 1 (HMSO, 1971).
- ⁵ *Third Report of the Council for Scientific Policy*, Cmnd 5117, Appendix E (HMSO, October 1972).
- ⁶ *Report on Science Policy*, Cmnd 3007 (HMSO, 1966).
- ⁷ *Second Report on Science Policy*, Cmnd 3420 (HMSO, 1967).
- ⁸ Cmnd 5117, *op. cit.*
- ⁹ *Education and Science in 1968*, Cmnd 3950, 93 (HMSO, 1969).
- ¹⁰ *A Framework for Government Research and Development*, Green Paper, Cmnd 4814 (HMSO, 1971).
- ¹¹ *Report on the Natural Environment Research Council*, HC 400, Appendix 10 (HMSO, 1969).
- ¹² Cmnd 3420, *op. cit.*, paragraph 7.
- ¹³ *Reviews of National Science Policy: United Kingdom and Germany*, 145 (OECD, Paris, 1967).
- ¹⁴ Cmnd 3420, *op. cit.*, paragraph 45.
- ¹⁵ Langrish, J., Gibbons, M., Evans, W. G., and Jevons, F. R., *Wealth from Knowledge*, 33 (Macmillan, 1972).
- ¹⁶ Cmnd 5117, *op. cit.*, paragraph 14.
- ¹⁷ Cmnd 5117, *op. cit.*, Appendix D, paragraph 22.
- ¹⁸ *Ibid.*
- ¹⁹ Cmnd 5117, *op. cit.*, paragraph 7.
- ²⁰ HC 522, *op. cit.*, question 5.
- ²¹ *Minutes of Evidence on Research and Development*, HC 114, 257 (HMSO, 1972).
- ²² HC 522, *op. cit.*, questions 17 and 18.
- ²³ *The Proposed 300 GeV Accelerator*, Cmnd 3503 (HMSO, 1968).
- ²⁴ HC 114, *op. cit.*, questions 263 to 271.
- ²⁵ *Hansard*, 769, column 830 (July 24, 1968).
- ²⁶ HC 114, *op. cit.*, question 1258.
- ²⁷ HC 114, *op. cit.*, question 256.
- ²⁸ Cmnd 3420, *op. cit.*, paragraph 66.
- ²⁹ Cmnd 4814, *op. cit.*, paragraph 41.
- ³⁰ HC 114, *op. cit.*, page 55.
- ³¹ Cmnd 4814, *op. cit.*, paragraph 36.
- ³² HC 114, *op. cit.*, question 31.
- ³³ HC 114, *op. cit.*, questions 1044 to 1046.
- ³⁴ *Reviews of National Science Policy: United Kingdom and Germany*, 142 (OECD, Paris, 1967).
- ³⁵ Cmnd 5117, *op. cit.*, Appendix C, paragraph 40.
- ³⁶ Cmnd 5046, *op. cit.*, paragraph 49.

Post-fertilization Synthesis of Polyadenylic Acid in Sea Urchin Embryos

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Sea urchin embryogenesis is accompanied by polyadenylic acid [poly(A)] synthesis. Inhibition of protein synthesis and prevention of the first cleavage result in a significant increase in poly(A) content, suggesting that poly(A) synthesis occurs immediately after fertilization.

MOLECULAR hybridization of unlabelled cytoplasmic RNA with ^3H -poly(U) under stringent reaction conditions¹ has been used to demonstrate that the inception of sea urchin embryogenesis is accompanied by a greater than two-fold net synthesis of polyadenylic acid [poly(A)], a species of RNA associated with metazoan genetic messages²⁻¹⁶ and certain viral RNAs^{1,4,15,17-22}. This represents the first demonstration of the net synthesis of an RNA moiety in response to fertilization. The post-fertilization synthesis of poly(A) is also immune to tandem suppression of transcription and significantly stimulated by the inhibition of protein synthesis. These results may suggest that the controlled adenylation of pre-existing genetic transcripts is modulated by a post-fertilization translational event other than the actual synthesis of the pertinent polymerase(s). It is interesting that this first demonstration of net synthesis of RNA following fertilization should implicate a species of nucleic acid believed to be regulatory rather than codogenic.

Fidelity of Poly(A) Assay

Results reported elsewhere have verified the quantitative reliability of the poly(A) assay used with regard to relatively homogeneous RNA populations such as viral RNAs¹. We now present evidence that a similar quantitative fidelity can be achieved with heterogeneous cellular RNAs containing vast excesses of non-complementary sequences (Fig. 1). Incubation at 36° C in the presence of 50% formamide, in conjunction with the known thermal instability of poly(A)-poly(U) duplexes, minimized any contribution of fortuitous complexes²⁵⁻²⁷. It is unlikely that poly(A) sequences less than 20 nucleotides long would form stable duplexes under these conditions²⁵.

The hybrids formed were resistant to simultaneous or sequential incubation with DNAase, pancreatic RNAase and

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T_1 RNAase. Alkaline digestion of unlabelled RNA before annealing abolished hybrid formation (Fig. 1). Conversely, DNAase treatment of unlabelled egg or embryonic RNAs followed by deproteinization of the RNA before annealing with ^3H -poly(U) did not affect the observed titres of poly(A). Non-complementary polyribonucleotides such as poly(G) neither hybridized with poly(U) (Fig. 1), nor altered the kinetics of hybridization between poly(U) and cellular RNA (not shown). The linear relationship between the input RNA and hybrid titre as well as the efficiency of conversion of ^3H -poly(U) (Figs. 1 and 2) to stable duplex clearly demonstrate that the presence of an excess of non-complementary RNA does not influence the observed titres of poly(A). No stable hybrids were formed between ^3H -poly(U) and bulk *E. coli* RNA prepared according to the pH 9.0 procedure (Fig. 1). These findings indicate that the poly(A) titres reported are not attributable to DNA contamination or aberrant base pairing, and that the assay conditions used accurately reflect the poly(A) ($n > 20$) titres of the RNAs in question. In agreement with the findings of Lee *et al.*⁶, preparation of RNA by the pH 6.0 procedure²⁴ resulted in a variable loss of poly(A)-containing RNA (Fig. 2), whereas the extraction procedure we used yielded poly(A) titres which varied by less than 7% for any developmental stage.

As sea urchin eggs and embryos contain a spectrum of stable genetic transcripts^{28,29-32} synthesized during oogenesis, and undergo alterations in permeability to nucleic acid precursors following fertilization^{33,34}, attempts to characterize their RNA populations by *in vivo* labelling and subsequent RNA/DNA hybridization are particularly sensitive to experimental uncertainties. The RNA-RNA hybridization used here to assess poly(A) titres of eggs and embryos clearly circumvents such ambiguities.

Fertilization Elicits a Net Synthesis of Poly(A)

While it is generally agreed that transcription is initiated during sea urchin embryogenesis before the four-cell stage^{24,35-38}, the negligible chemical contribution of these transcripts to the pre-existing population of RNA makes such synthesis difficult to detect^{39,40} unless high levels of labelled precursors are employed. Indeed, early development appears to be characterized by an overall reduction in total RNA content⁴¹. Fig. 2 shows, however, that the poly(A) titre characteristic of the unfertilized egg increases two-fold between fertilization and the two-cell stage.

The size of the poly(A) sequences synthesized (see below) precludes any involvement with tRNA. Alternatively, the possibility that the observed synthesis of poly(A) is associated with rRNA is unlikely as sedimentation profiles⁴², electrophoretic mobilities²⁴ and base composition⁴² of sea urchin ribosomal RNA prepared from eggs and embryos are very similar. Evidence confirming the absence of

poly(A) sequences in sea urchin rRNA capable of hybridizing with poly(U) will be presented elsewhere⁴³. As very little nuclear RNA is synthesized between fertilization and the four-cell stage⁴⁰, and as the nuclear fractions were removed during preparation, it is unlikely that the newly synthesized poly(A) is added to the nuclear RNA. The possibility that the poly(A) is physically autonomous is eliminated by our finding that the newly synthesized sequences are associated with RNAs possessing molecular weights in excess of the poly(A) regions *per se*⁴³. These results, findings to be reported elsewhere⁴³, as well as other evidence³⁻¹⁶ showing that cytoplasmic poly(A) is a characteristic of translatable metazoan RNA, support the conclusion that this poly(A) is an adjunct of genetic messages.

Poly(A) Titres and Post-fertilization Protein Synthesis

The existence of latent genetic messages in the unfertilized egg^{23,30,32} and the fact that inhibition of transcription does not inhibit the increase in protein synthesis^{28,31,32} as well as polysome content⁴⁴⁻⁴⁶ which normally follows fertilization imply that this activation initially depends on the conscription of pre-existing RNA transcripts. The proposed role of poly(A) in the processing of genetic messages^{3,4,6-10,12,15} invited speculation that the lack of protein synthesis in the ovum might be due to a deficiency in poly(A). Assessment of the poly(A) content of RNA from unfertilized eggs failed to support this (Figs. 2 and 3). Potential genetic messages

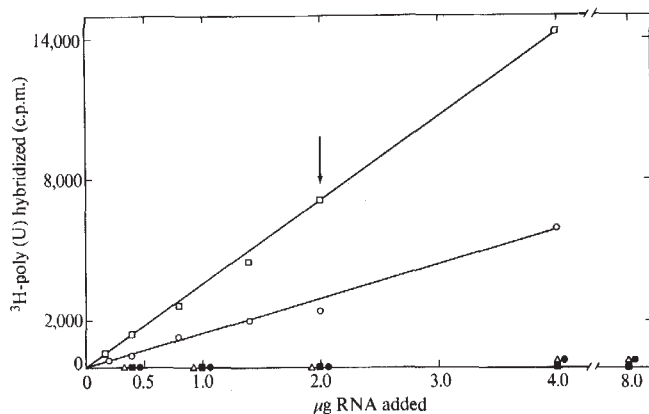


Fig. 1 Regression hybridization of RNA. Hybridization was carried out in the formamide-SSC system¹ in 60 µl. at 36° C for 24 h, using 8 ng of ³H-poly(U) (3.9×10^6 c.p.m. µg⁻¹) and the indicated amount of RNA. Processing of the hybrids involved the elimination of unpaired ³H-poly(U) by RNAase A and DNAase treatment followed by acid precipitation as previously described¹. Where used alkaline digestion was carried out by treatment with 0.4 M NaOH at 30° C for 20 h and subsequent neutralization with HCl before use for hybridization. *E. coli* RNA was prepared from spheroplasts by repeated extractions with phenol-cresol-chloroform at pH 9.0 according to a modification of the pH 9.0 procedure¹¹ (to be published separately). The inability of DNAase digestion before hybridization to influence the yield of RNAase-resistant hybrid precludes any possible contribution of residual DNA to the poly(A) titres obtained. Radioactive poly(U) was synthesized in a reaction mixture consisting of 0.1 M Tris, pH 8.2; 7.5 mM MgCl₂; 1 mM EDTA; 178 µg ml⁻¹ of polynucleotide phosphorylase (Worthington Biochemical Corporation); and 180 µg ml⁻¹ of ³H-uridine diphosphate, 2.9 Ci mmol⁻¹. Following incubation at 30° C for 16-20 h the mixture was made 0.5% with sodium dodecyl sulphate (SDS). The ³H-poly(U) was extracted once with 1 vol. of chloroform containing 0.1 vol. of isoamyl alcohol, made 0.2 M with NaCl, and precipitated with 2 vol. of ethanol at -20° C. Subsequent to centrifugation at 12,000g the precipitate was dissolved in 40 µl. of water for each 1 mCi of UDP input. After adding an equal vol. of ethanol, the solution was stored at -20° C. Approximately 2-10% of the UDP was normally converted to acid-precipitable radioactivity and 50% of the radioactivity in the final solution was acid-precipitable. On polyacrylamide gels, the polymer exhibited a heterogeneous size distribution ranging from 2S to 10S. Eggs and sperm were obtained from *Lytechinus pictus*²³ and embryos were cultured in spinner flasks at 18° C²⁴. Control cultures displayed greater than 95% normal development through gastrulation. Eggs and embryos were washed in 1 M dextrose and resuspended in a solution containing 0.3 M KCl, 0.002 M MgCl₂ and 0.05 M Tris-HCl, pH 8.0, at 25° C, made 0.2% (v/v) with 'Triton X-100'. After homogenization in a 'Dounce' homogenizer the homogenate was centrifuged at 15,000g for 10 min to remove nuclei and particulate cell debris. The resulting supernatant was precipitated at -20° C with 2 vol. ethanol, centrifuged at 20,000g for 30 min and thoroughly drained. Following resuspension in 0.1 M Tris-HCl, pH 9.0, made 0.5% with SDS, RNA was extracted from eggs or embryos essentially according to the non-sequential pH 9.0 procedure of Lee *et al.*⁶, modified to include re-extraction of the initial two phenol layers and interphases as well as the final combined aqueous phases with 0.5 vol. of chloroform. The last aqueous phase was then made 0.1 M with NaCl, precipitated with 2 vol. of ethanol at -20° C and dissolved in a solution containing 0.1 M NaCl and 0.05 M sodium acetate, pH 6.0. After two such ethanol cycles, the RNA was stored at -70° C. All sea urchin RNA preparations utilized displayed A_{260nm}/A_{230nm} and A_{260nm}/A_{280nm} ratios of greater than 2.0. □, 4-cell RNA; ○, unfertilized egg RNA; △, poly(G); ■, NaOH-treated 4-cell RNA; ●, *E. coli*, pH 9, RNA.

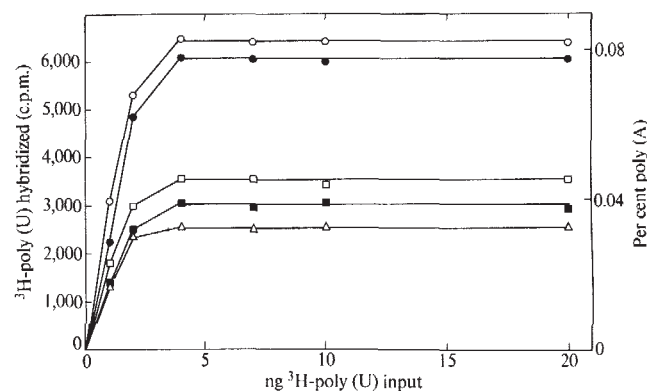


Fig. 2 Saturation hybridization with egg and embryonic RNA. RNA from eggs and embryos was prepared as described in Fig. 1. Extraction of RNA at pH 6.0 was carried out as described elsewhere²⁴ with the omission of the 4 M NaCl precipitation. Hybridization conditions were as described in Fig. 1, except that the amount of unlabelled RNA was held constant at 2 µg while that of ³H-poly(U) (3.9×10^6 c.p.m./µg) was as indicated. ○, 4-cell RNA; ●, 2-cell RNA; □, 60 min post-fertilization; ■, 30 min post-fertilization; △, unfertilized egg.

appear to comprise between 3 and 4% of the RNA native to the ovum^{23,30}. Assuming for illustration that all mRNA contains poly(A) sequences, and employing the 4% value, the 0.032% poly(A) titre observed for the unfertilized egg would infer a poly(A) content of approximately 0.8% for the maternal message population. By analogy with other cellular RNAs assayed by the procedure used here, the poly(A) titre of the unfertilized egg message population seems sufficient to support active protein synthesis. The translational inactivity of the unfertilized egg therefore cannot be simply due to a quantitative deficiency in poly(A). The longevity of the latent messages in the unfertilized egg implies that adenylation of a genetic message can occur well before its translation. Thus, either the degree of adenylation is critical for

message capacitation, or the addition of poly(A) is not sufficient for the message to function as a template.

The increase in incorporation of amino-acids into TCA precipitable material^{32,47,48} and the genesis of polysomes⁴⁹ are approximately linear between fertilization and the four-cell stage. In comparison, the major increase in poly(A) occurs between syngamy and the two-cell stage when it reaches a level over twice that of the ovum (Fig. 3). As subsequent development to the four-cell stage does not result in any significant increase in poly(A), such synthesis does not appear to be correlated with mitosis. The data therefore indicate that poly(A) and protein synthesis are not coincident although both are initiated within the same development period.

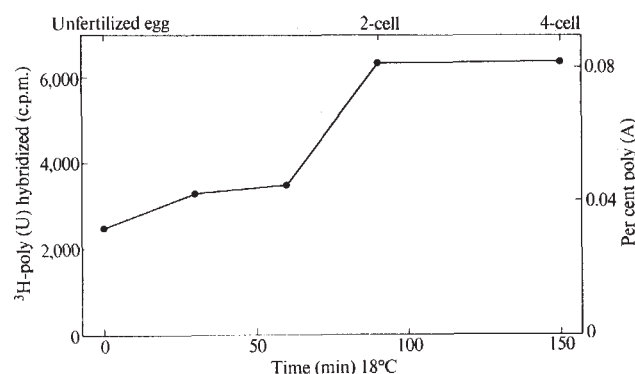


Fig. 3 Post-fertilization transitions in poly(A) titres. Hybridization was performed under conditions of ³H-poly(U) saturation employing 16 ng per 2 µg of the indicated unlabelled RNA. All other procedures were as given in Fig. 1.

In addition to its proposed function in the processing of mRNA, poly(A) may be involved in censoring the translation of metazoan genetic messages^{1,19,22}. Bulk RNAs from unfertilized eggs or embryos with distinguishable poly(A) titres display identical message potentials, however, when used as templates in an *E. coli* cell free amino-acid incorporating system calibrated with MS2 RNA as a primary standard^{23,30}. RNA from MS2¹, Qβ¹, T-4¹⁵ as well as *E. coli* RNA (ref. 5 and Fig. 1) do not, however, contain appreciable titres of poly(A) when assessed by the ³H-poly(U) assay or with poly(U) filters and columns. Utilization of templates by the *E. coli* translational apparatus therefore does not appear contingent on poly(A). Hence, template titres obtained with an *E. coli* cell-free system, although presumably capable of reflecting the codogenic capacities of an RNA population⁵⁰, would not be expected to discern differences in poly(A) titres characteristic of the eggs and embryos in question.

Poly(A) Content of Nitrocellulose Membrane-bound RNA

Using values of 4% (ref. 30) and 0.082% (Figs. 2 and 3) for the respective message and poly(A) content of four-cell embryos, and assuming again that all messages are associated with poly(A), implies that the message population native to these embryos contains 2.1% poly(A). Lower estimates of message content or the presence of messages devoid of poly(A) would increase this figure.

Genetic messages containing poly(A) sequences can be selectively bound to nitrocellulose filters^{6,11,16}. This permitted assessment of the validity of our estimates of 0.8% and 2.1% for the poly(A) titres of message populations native to egg and four-cell embryo respectively, as well as determining

whether this transition resulted in any increase in the quantity of RNA bound per unit of input RNA. Although filtration failed to remove all the poly(A) from the filtrate, it did effect a greater than 26-fold enrichment in the poly(A) content of both unfertilized egg and four-cell embryo RNA (Table 1). In contrast to the similarity in enrichment, the poly(A) titres of the RNAs absorbed were decidedly different with a resulting ratio of four-cell embryo to unfertilized egg RNA of 3.1. This ratio is therefore in reasonable agreement with our projected value of 2.6. Optical density determinations of the amount of membrane-bound RNA further indicated that approximately three times more embryonic RNA was retained on the filters than unfertilized egg RNA when identical inputs of RNA were used. As previously noted, the quantitative contribution of RNA transcribed between fertilization and the four-cell stage is minimal. We therefore suggest that this net increment in membrane-bound RNA and its increased poly(A) titre result from the post-transcriptional addition of poly(A) to pre-existing genetic messages.

Post-fertilization Increase in Size of Poly(A) Tracts

Polyacrylamide gel electrophoresis of nuclease-resistant hybrids formed between ³H-poly(U) and unfertilized egg or four-cell RNA indicated that the poly(A) sequences present in the respective RNA populations were quite dissimilar with respect to size (Fig. 4). As this poly(A) assay allows visualization of total poly(A) content rather than the limited representation realized with pulse-labelled RNA, direct comparisons with results obtained by others employing pulse-labelled RNA are not permissible. However, the electrophoretic profile of the duplexes observed with unfertilized egg RNA is, given the egg's relative metabolic inactivity, surprisingly similar to poly(A) sequences native to rapidly proliferating adult tissues when assayed by the same procedure. On the basis of the electrophoretic mobilities of haemoglobin poly(A) sequences and viral RNAs possessing poly(A) tracts of known molecular weights, we estimate the mean composition of the unfertilized egg RNA poly(A) sequences to be approximately 100 nucleotides.

Table 1 Poly(A) Content of Nitrocellular Membrane-bound RNA

RNA	C.p.m./µg RNA Total	C.p.m. absorbed C.p.m. total	% poly(A) of RNA absorbed
Unfertilized egg	1,250	32,331	26
Four-cell	3,268	101,464	31

The method used was essentially identical to that described by Lee *et al.*⁶, and Brawerman *et al.*¹¹. A total of 1.5 mg of either unfertilized egg RNA or four-cell RNA was adjusted to a final concentration of 150 µg ml.⁻¹ in a solution consisting of 0.5 M KCl, 0.001 M MgCl₂, and 0.01 M Tris-HCl, pH 7.5. RNA was passed through nitrocellulose filters (0.45 µm, 25 mm) at approximately 0.5 ml. min⁻¹ at 24° C. After loading, the filters were serially washed at 4° C with a total of 5 ml. of 0.1 M Tris-HCl, pH 9.0, made 0.5% with SDS. The combined washes were subsequently centrifuged twice at 15,000g for 10 min to remove SDS, made 0.1 M with NaCl and precipitated with 2 vol. of ethanol at -20° C. The resulting precipitate was washed three times with a solution containing 75% ethanol made 0.1 M with respect to NaCl, dissolved in NC and stored at -70° C. Hybridization was carried out as described in Fig. 2.

In contrast, the poly(A)-rich tracts present in four-cell RNA are decidedly unique in size and homogeneity. Only RNA from oncogenic viruses displays a similar electrophoretic profile¹. Indeed, the mobilities of the four-cell RNA poly(A) tracts are essentially the same as those in oncogenic viruses which also contain discrete poly(A)

sequences of approximately 200 nucleotides¹. Analysis of the physical characteristics of the egg and embryonic adenylated RNA⁴³, however, precludes ascribing either this similarity in the size of the poly(A) tracts or the synthetic RNA-directed DNA polymerases present in these embryos⁵¹ to viral contamination.

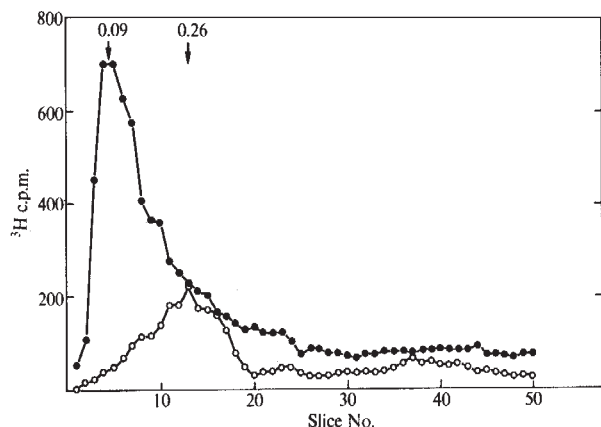


Fig. 4 Polyacrylamide gel electrophoresis of poly(U)-poly(A) hybrids. Hybridization was carried out in a 300 μ l. reaction, using 4 μ g of the unlabelled RNA indicated and 16 ng of ³H-poly(U). Nuclease treatment was carried out as previously described, but for 2 h and without T₁ RNAase¹. After adding SDS to a final concentration of 0.5%, the solution was extracted with 1 vol. chloroform and 0.1 vol. isoamyl alcohol, and the aqueous phase was precipitated with 2 vol. ethanol at -20° C. The precipitate was collected by centrifugation at 13,700g for 2 h and gel electrophoresis was carried out as previously described¹. ●, Four-cell; ○, egg.

Comparison of the size of the poly(A) tracts native to unfertilized egg and four-cell RNA poses the question as to whether the smaller, more heterogeneous poly(A) sequences present in unfertilized egg RNA are catabolic products resulting from their utilization during oogenesis, or, alternatively, anabolic intermediates in the process of completion. The size of the four-cell RNA poly(A) sequences in conjunction with the three-fold increase in poly(A)-associated RNA is incompatible with the catabolic concept. We therefore interpret these results as suggesting the presence of unadenylated or partially adenylated genetic messages in the unfertilized egg which are completed subsequent to fertilization.

Poly(A) Synthesis Does Not Require Tandem Transcription

Actinomycin D quantitatively suppresses transcription in sea urchin embryos without inhibiting either the increase in the rate of protein synthesis^{29,52} or the formation of poly-somes⁴⁴⁻⁴⁶ which follows fertilization. The establishment of embryonic competence⁵³ and development through cleavage also continue²⁹. Although continuous exposure to actinomycin D resulted in blastula arrest, removal of the drug before the third cleavage allowed normal development through gastrulation. As actinomycin D-mediated suppression of transcription is not solely dependent on the presence of deoxyguanosine or binding to DNA⁵⁴, it might be expected to diminish DNA-dependent poly(A) synthesis. Alternatively, if poly(A) synthesis was dependent on the tandem transcription of new primers, the relatively high concentration of actinomycin D used would obliterate such synthesis. We found that actinomycin D was totally in-

capable of inhibiting poly(A) synthesis (Fig. 5 and Table 2), thus establishing that the tandem transcription of new primers is not essential to poly(A) synthesis and further suggesting that the poly(A) tracts may not be directly transcribed from DNA.

Ethidium bromide also did not alter the titre of poly(A) (Fig. 5 and Table 2), eliminating the involvement of mitochondrial transcription and again suggesting that the poly(A) is not a product of nuclear DNA-dependent RNA synthesis.

3'-Deoxyadenosine partially suppresses poly(A) synthesis^{10,12}. Development of embryos in sterile artificial sea water containing 3'-deoxyadenosine at 20 μ g ml.⁻¹ or 50 μ g ml.⁻¹ resulted in blastula arrest. In contrast to either actinomycin D, ethidium bromide or 2'-deoxyadenosine (see below) the developmental block could not be reversed by removal of the 3'-deoxyadenosine at the four-cell stage. At either concentration this irreversible effect on development was paralleled by a 14% reduction in the titre of poly(A) (Fig. 5 and Table 2). The inability of four-cell embryos to recover from their relatively brief exposure to 3'-deoxyadenosine precludes us ascribing the comparative immunity of poly(A) synthesis to a permeability function. As ATP is the predominant constituent of the acid-soluble nucleotide pool present in sea urchin eggs and early embryos⁵⁷, we initially attributed the 3'-deoxyadenosine resistance of poly(A) synthesis to competition between the analogue and its natural congener. However, this interpretation may not sufficiently explain the action of 3'-deoxyadenosine.

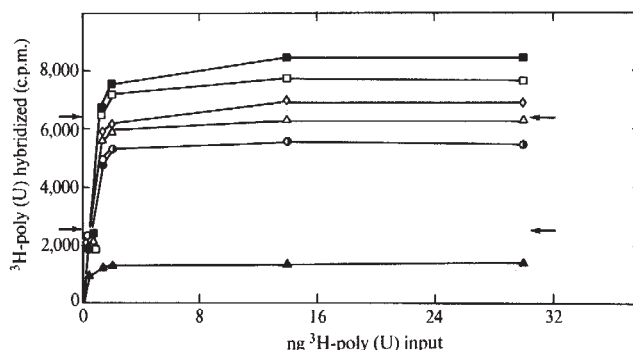


Fig. 5 Saturation hybridization of RNA from embryos treated with antimetabolites. Three min after fertilization the zygotes were chilled to 4° C and serially washed into sterile artificial sea water containing the antimetabolites indicated at the following concentrations: actinomycin D, 20 μ g ml.⁻¹; ethidium bromide, 5 μ g ml.⁻¹; 3'-deoxyadenosine, 20 μ g ml.⁻¹ (○) or 50 μ g ml.⁻¹ (●); 2'-deoxyadenosine, 50 μ g ml.⁻¹; and emetine, 55 μ g ml.⁻¹. The embryos were then cultured at 18° C as described in Fig. 1. Except for the emetine-treated zygotes, the embryos were collected at the four-cell stage. As emetine arrested development before first cleavage, these zygotes were collected when the appropriate controls reached the four-cell stage. RNA was, in all cases, prepared according to the procedure as shown in Fig. 1. Hybridization conditions were as described for Fig. 2. The lower and upper arrows indicate the poly(A) titres of untreated unfertilized egg RNA and 4-cell RNA respectively. ■, Emetine; □, actinomycin; ◇, 2'-deoxyadenosine; △, ethidium bromide; ●, 3'-deoxyadenosine; ▲, pH 6.

To determine whether these effects of 3'-deoxyadenosine were merely a consequence of the general toxicity of adenine derivatives, embryos were developed in 2'-deoxyadenosine at 50 μ g ml.⁻¹. Prolonged exposure to 2'-deoxyadenosine blocked development at early- to mid-gastrula. This effect was reversible if the metabolite was removed at the four-cell stage. No inhibition of poly(A) synthesis resulted from 2'-deoxyadenosine treatment (Fig. 5 and Table 2). The

irreversibility of 3'-deoxyadenosine-mediated developmental arrest and the marginal reduction in poly(A) titre therefore cannot be mimicked by 2'-deoxyadenosine even when employed at 2.5-fold the concentration of 3'-deoxyadenosine.

Inhibition of Translation Stimulates Poly(A) Synthesis

Emetine is a particularly potent inhibitor of protein synthesis in sea urchin embryos which irreversibly suppresses amino-acid incorporation by greater than 90% after 5 min exposure and irreversibly halts development⁵⁸ at the concentrations used here. As it uniformly prevented first cleavage, the arrested embryos were collected when the appropriate controls reached the four-cell stage. Morphologically these uncleaved zygotes mainly resembled their normal 30-min counterparts. The poly(A) content of the emetine-treated embryos was, however, approximately 2.5-fold that of either the untreated 30-min or 60-min zygotes and 1.3-fold that of four-cell embryos (Figs. 3 and 5, and Table 2). As the maximal variation in poly(A) content encountered between normal cultures collected at the four-cell stage was less than 7%, the increase resulting from exposure to emetine is thought significant. Given the rapidity with which emetine suppresses translation, it is also apparent that either the polymerase(s) involved are prematurely synthesized immediately after fertilization, or that fertilization activates pre-existing enzyme(s). The evidence available, which indicates that although the mature ovum is not actively engaged in nucleic acid synthesis it does contain potential polymerases in amounts quantitatively comparable to that of the early embryo (ref. 51, and I. S. and D. W. S., in preparation), suggests the latter interpretation. Indeed, the increased poly(A) titre of emetine-treated embryos suggests that poly(A) synthesis is actually restricted by the translational activity which normally attends fertilization.

Table 2 Effect of Antimetabolites on Poly(A) Synthesis

Addition	C.p.m. ³ H-poly(U) hybridized/μg RNA	% poly(A)
None	3,214	0.082
Actinomycin D	3,819	0.097
Ethidium bromide	3,133	0.080
3'-Deoxyadenosine	2,752	0.071
2'-Deoxyadenosine	3,446	0.088
Emetine	4,205	0.108

Zygotes were incubated with the indicated antimetabolite as described for Fig. 5. RNA was prepared as detailed in Fig. 1. The hybridization procedure used was as presented in Fig. 2.

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Evidence for Global Synchronism in Mantle Plume Convection, and Possible Significance for Geology

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Volume estimates and age dating of basic, non-orogenic igneous deposits suggest synchronous discharge from many hot spots, identified as mantle plumes. The discharge fluctuations seem to mirror the histories of diverse global geological parameters. Extinction crisis of faunal evolution may reflect trace-element pollution occurring during intervals of intense vulcanism reflecting active plume convection.

Hot spots¹⁻⁴ have been hypothetically identified as mantle plumes⁵⁻⁷ and their stationarity with respect to the Earth's spin axis demonstrated⁵⁻⁸. Can the discharge of plume material into the asthenosphere be somehow estimated, and, if so, do different plumes discharge at random, or in such a way as to maintain constant total discharge, or in synchronism? I have discussed this question at length elsewhere⁹; here I present a briefer discussion.

If plumes do constitute at least a partial motor for plate tectonics, the history of plume discharge (mass transferred from the lower to the upper mantle per unit time) should relate to the kinetics of crustal plates, which at least for the past 80 m.y. has been fairly well documented^{10,11} and is being continually refined. The discharge histories of the roughly two dozen plumes could conceivably fall into any of several patterns. Discharge could vary randomly from one plume to another; with global synchronism, most plumes waning and waxing in discharge rate more or less in unison; with regional synchronism; pseudo-randomly, in such a way that total discharge remains nearly constant; or with local or regional compensation, adjacent plumes or groups of plumes varying their

discharge out of phase with each other, rather like separate vents of a single volcano.

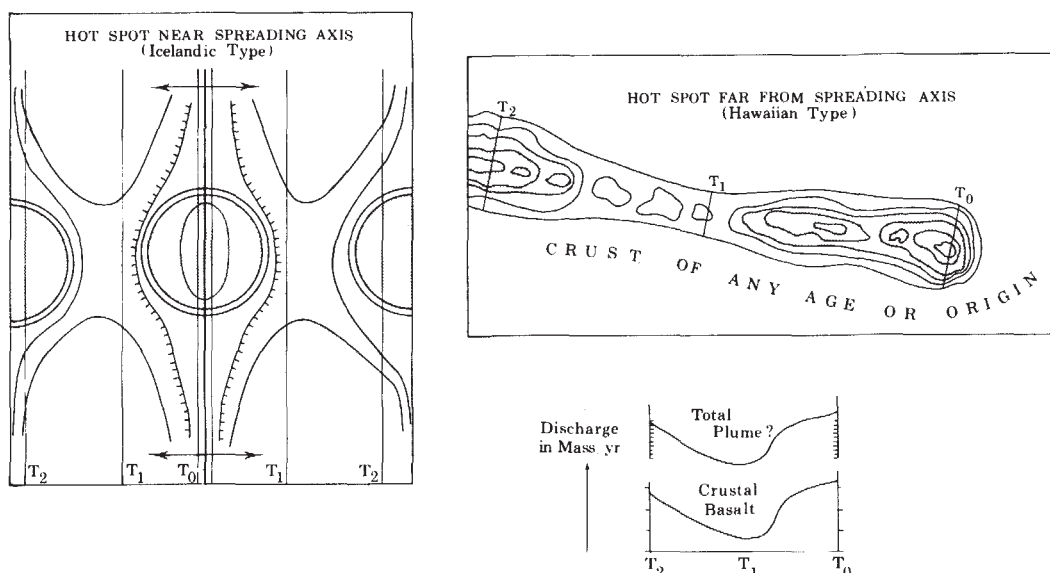
To discriminate among these possible discharge models, a method of estimating discharge must first be defined. Of the large quantity of ultrabasics transferred into the asthenosphere by a typical plume ($\sim 25 \text{ km}^3 \text{ yr}^{-1}$) only a small basaltic fraction is deposited on or in the lithosphere in the form of intrusives and volcanics. Total discharge is not accessible to surface observation, but the basaltic fraction can be measured subject to a number of conditions, such as survival from erosion⁹. If basaltic discharge can be assumed to be roughly proportional to total discharge, then discharge histories for both "Icelandic" and "Hawaiian" plumes are in principle simply a matter of volume measurement and age dating (Fig. 1). With "Icelandic" plumes, isotope dating along the two limbs of the aseismic ridge may be supplemented by palaeontological dating by the deep-drilling programme, and by projected crustal isochrons derived from magnetic anomalies. Oceanic plumes—all Icelandic but only some Hawaiian ones—are spared most or all of subaerial erosion⁹. Thus, oceanic plumes are more suitable for discharge estimates. Offsetting this advantage is the fact that oceanic plume deposits are subducted within 10 to 200 m.y. after formation. For ancient continental plumes, the only index of discharge activity may be relative frequency of isotope dates.

Synchronous Plume Discharge Histories for Iceland and Hawaii?

Although volume integrations (Fig. 1) have been applied to numerous plume trails, age dating and geophysical information are still quite scarce in most cases. The Hawaiian-Emperor (Fig. 2) and Greenland-Iceland-Faeroe (Fig. 3) aseismic ridges are among the best studied, so I emphasize them here.

The discharge curve for the Hawaiian-Emperor chain (Fig. 4) was derived by dividing the chain into 50 km increments, estimating the volume in each increment, and applying

Fig. 1 Variations in total plume discharge may be reflected by variations in discharge of basaltic derivatives into or onto the lithosphere. Here the effect of a simple variation pattern on an Icelandic (ridge centred) and a Hawaiian (plate centred) hot spot is shown. Contours show thickness of excess basaltic crust, or, as an approximation to this quantity, the height of the volcanic basement above the normal regional level. Two pairs of V-shaped scarps are shown associated with the recent increase in discharge from the Icelandic plume¹⁷.



a smoothed age scale based on dates reported by Jackson *et al.*¹² and the Deep Sea Drilling Program¹³. Morgan's⁵ 40 m.y. BP estimate for the age of the Hawaii-Emperor bend is preferred over the 25 m.y. BP extrapolation¹², because this fits the overall trend of the Hawaii-Emperor dates. It seems probable that plate motion over the mantle was generally faster during times of higher discharge; so the assumption of constant rate of plate motion will tend to produce a conservative discharge curve. The volume estimates include a slight correction for the amount lost to sub-aerial erosion, and a 20% addition for the crustal basaltic root, below the regional top of the oceanic basement. The root was estimated from the seismic refraction and reflexion experiments on Hawaii, Oahu, and Gardner Pinnacles¹⁴.

Individual data points for the Hawaiian chain show considerable scatter, reflecting the fact that the Hawaiian ridge is really a chain of discrete volcanic centres. It is unlikely that short period time variations are responsible for this phenomenon. Instead, the scatter may indicate that the individual shields form at fractures or fracture intersections in the lithosphere, a hypothesis which may explain the 50 to 100 km shield separation observed along the Hawaii-Emperor¹² and other island chains⁹.

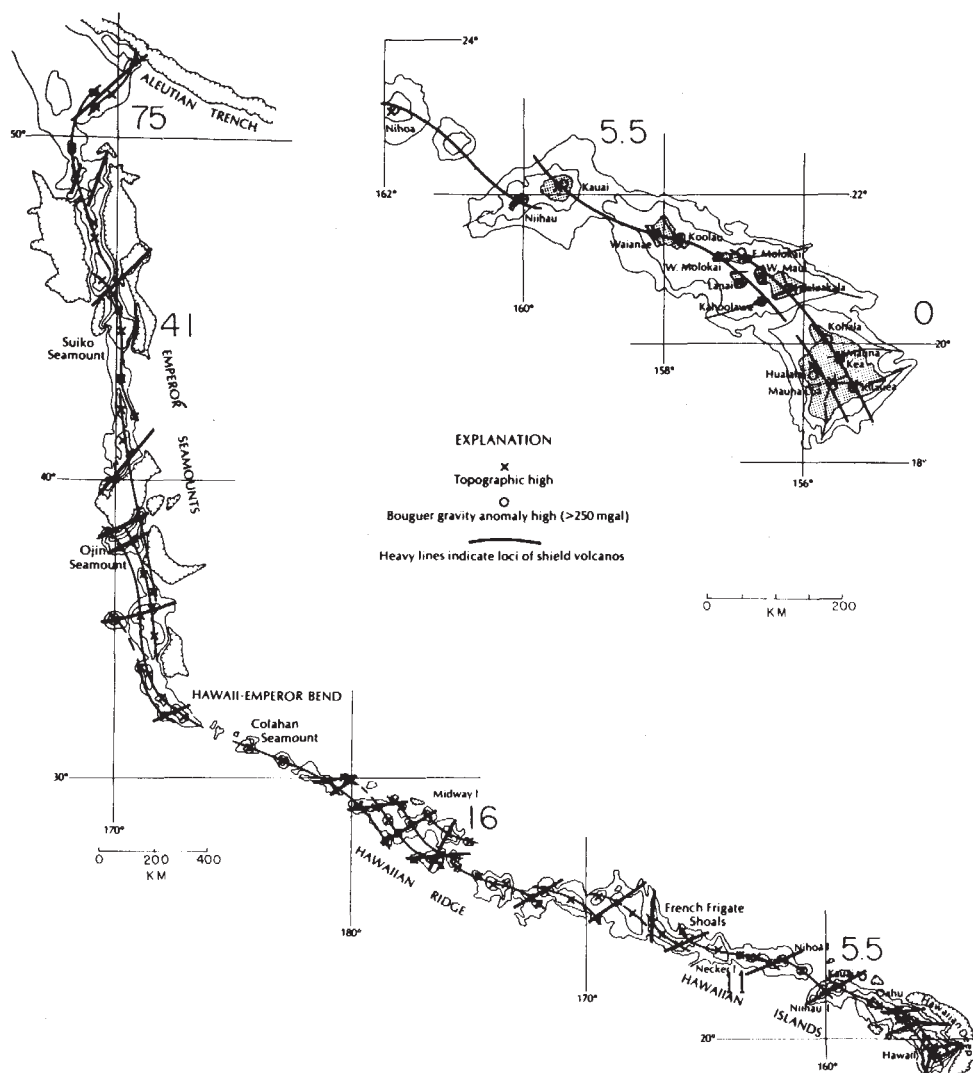
Long term variations are easily recognizable for the Hawaiian curve, however, and I propose that these represent real fluctuations in total discharge from the Hawaii plume. The overall pattern is one of high discharge in the late Cretaceous and early Tertiary, followed by a low in the mid-Tertiary and a renewed increase in the late Tertiary. Superimposed on this are two additional discharge highs, at 15 m.y. BP and approximately 40 m.y. BP. Discharge has varied by at least an order of magnitude in the past 70 m.y. Large trend changes, reflecting

changes in motion of Pacific plate over the deeper mantle, should tend to occur near times of high or changing discharge if this discharge is a significant term in the equation of motion for the Pacific plate. Observations suggest that this is the case (Fig. 4). Discharge has increased to about $0.13 \text{ km}^3 \text{ yr}^{-1}$ in latest Tertiary times, a figure that is surprisingly close to Swanson's¹⁵ estimate of $0.11 \text{ km}^3 \text{ yr}^{-1}$ (non-vesicular equivalent) for the rate of lava supply from the upper mantle, based on a study of major eruptions of Kilauea between 1952 and 1971.

The discharge curve for Iceland was constructed (as in ref. 17) by extrapolating seafloor spreading isochrons¹⁶ from the ocean floor immediately southwest of the aseismic ridge. I also considered isotope dating of basalts from East Greenland¹⁸, the Faeroes¹⁹, and Iceland²⁰, and assigned an average crustal thickness of 12 km to the entire aseismic ridge based on the seismic refraction work on Iceland²¹ and the Faeroe-Iceland limb of the aseismic ridge²². Actually the thickness may increase from 8 km near Iceland to 20 km near the Faeroes.

A rough parallelism between the histories of the Iceland and Hawaii plumes is noteworthy (Fig. 4). The absence of the 40 and 15 m.y. BP discharge peaks on the Icelandic curves does not mean that these events did not affect the Iceland plume. The slow spreading and fairly wide volcanic zone would smear out events of short period. Both events seem to have been recorded by the Reykjanes Ridge, in the form of a pair of time-transgressive scarps that left the plume about 17 m.y. BP (ref. 17), and the reorientation from transform faults to oblique spreading, an event explicable in terms of increased plume flow, that began near Iceland about 40 m.y. BP. A geological record of the beginning of the late Cretaceous/early Tertiary discharge

Fig. 2 Bathymetry of Hawaii-Emperor chain, after Jackson *et al.*¹². I have added NE to E trending transverse fractures to their sigmoidal pattern. Numbers are some ages in m.y. BP. Note chain is wider, more massive near Hawaii, French Frigate shoals, south of Ojin seamount, and near Aleutian trench. x, Topographic high; O, Bouguer gravity anomaly high (>250 mgal); —, loci of shield volcanoes.



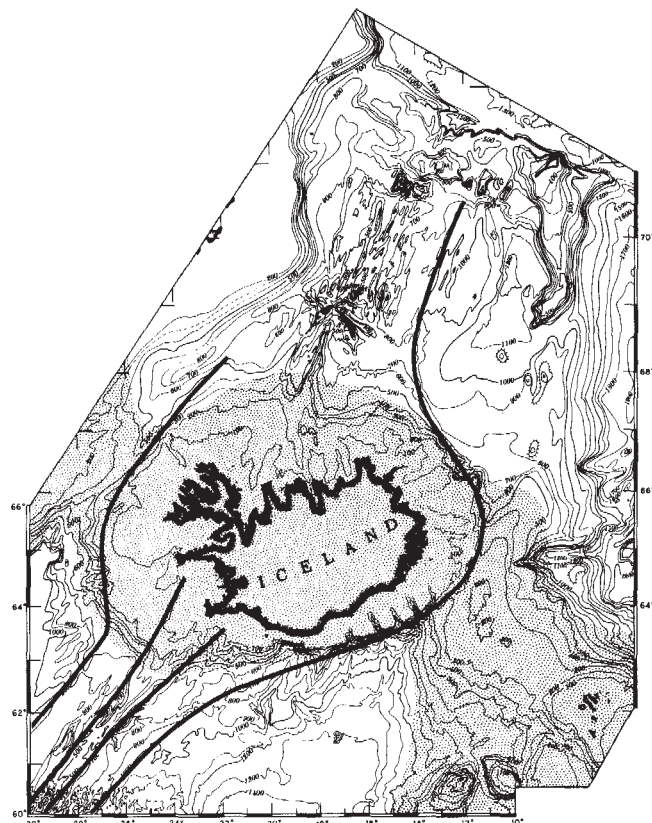


Fig. 3 Bathymetry of Greenland-Iceland-Faeroe aseismic ridge (dotted) suggests variable discharge schematized in Fig. 1. Heavy black lines are time transgressive trends, appearing on the Reykjanes Ridge as outward facing scarps, and suggesting at least two late Tertiary pulse-like increases in discharge¹⁷.

peak of the Hawaiian plume has already been subducted (Fig. 2), but isotope dates from the British Tertiary volcanic province begin around 75 m.y. BP (ref. 23), with the great mass of flood basalts probably extruded about 55 to 60 m.y. BP.

Evidence from Other Plumes

Discharge histories of the widely separated Iceland and Hawaii plumes (Fig. 4) support the "globally synchronous" model for plume convection. More limited data from other plumes⁹ also support this model; not all plumes recorded all peaks, but within the limits of available age dates and other information, all observed peaks were closely coeval. A late Miocene to Recent increase in plume volcanism affected at least the Azores, Canaries, Cape Verdes, and Madeira in the Atlantic; the Massif Central in France; the Afar area of Africa; and the Galapagos plume. The 15 m.y. BP event is evident in the volcanism of the Afar Triangle²⁴ and the Galapagos plume⁹; the Columbia basalts were extruded at this time (14–18 m.y. BP, ref. 25). The late Cretaceous-early Tertiary event caused widespread basic volcanism reflected by three groups of basement highs and seamounts of this age in the North Atlantic⁹, late Cretaceous-early Tertiary basalts in Portugal²⁶; 55 m.y. BP plateau basalts of Davis Straits in the Labrador Sea²⁷; the massive Kerguelen-Broken Ridge, produced by the Amsterdam plume about 50 m.y. BP; the great Deccan Traps, roughly 10^6 km³ of flood basalts extruded approximately 65–60 m.y. BP (ref. 28). The extensiveness of late Cretaceous/early Tertiary volcanism is also seen in the Eocene sediments of the coastal plain of the USA (ref. 29) and the 50–100 m thick volcanogenic layer deposited some time between late Cretaceous and early Tertiary and lying just below the Eocene cherts of the western North Atlantic basin³⁰. (Whether the layer was produced by hydrothermal exhalations or actual volcanic eruptions is immaterial here—in either case

exceptional igneous activity is indicated; the layer cannot be ascribed to hydrothermal activity of the sort supposed to be common at spreading axes, because the Mid-Atlantic Ridge was about 1,000 km distant at the time.)

Although the global nature of the 40 and 15 m.y. BP discharge peaks apparently recorded by the Hawaii plume may be questioned, the evidence points strongly to synchronous discharge peaks in the late Cretaceous/early Tertiary and again in the late Tertiary. The close correspondence in age of the Brito-Arctic early Tertiary flood basalts, the distant Deccan Traps, and the Davis Straits basalts is itself enough to arouse suspicion. Only in recent years has the radiometric dating tool been sharpened to the point where such conclusions can be made about the time of flood basalts. With increasing evidence for synchronicity of extrusion has come an indication that the basalts were poured out in geologically brief intervals, perhaps no more than 5 m.y. (refs. 18, 25, 28).

Correlation with "Global" Parameters

If plume convection fluctuates with a global rhythm, and if such convection contributes to driving plate tectonics^{5–7}, one might expect some correlation between discharge and diverse geological parameters of regional or global significance. In Fig. 5 several such parameters are plotted without regard to causal mechanisms. Although the disparate curves are subject to their own uncertainties and uncertainties in the time scale⁹, a certain degree of intercorrelation is suggested. (Other parameters have also been considered as in ref. 9; for example, spreading rate and plume discharge were both high in the late Cretaceous/early Tertiary, and again in the late Tertiary; new rift axes have tended to form during high plume discharge.) Even if these various correlations turn out to be real, their meaning in terms of causality poses some tremendous challenges. The following are merely suggestions.

Increased plume discharge could decrease global temperature: directly, by injecting more volcanic dust into the upper atmosphere; by inducing epeirogenic doming under continents, plume discharge could increase the continentality of climate; by driving the lithospheric plates at higher rates, island arc, continent-continent and continent-ocean compression and volcanism are more active; higher mountains in-

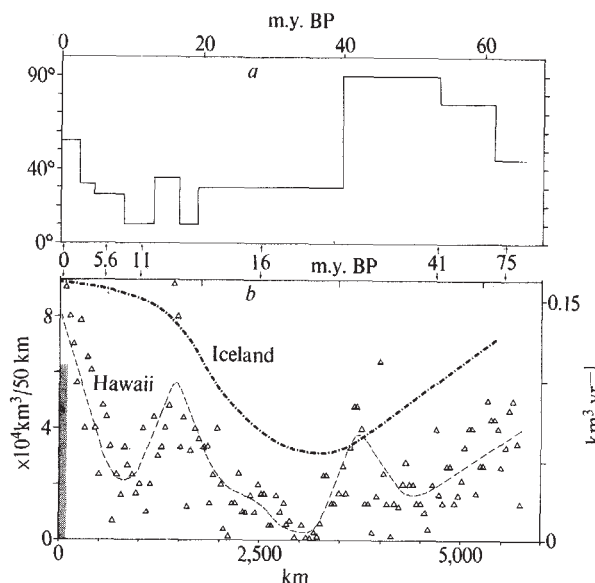


Fig. 4 *a*, Trend, with respect to west, against age along Hawaii-Emperor chain; *b*, Discharge history of Hawaiian and Iceland plumes. Derivation of curves described in text. Averaged linear time scale (top) was used to calibrate Hawaii discharge (lower right vertical scale). Measured ages along chain shown between *a* and *b*. Arrow marks position of Hawaii-Emperor bend. Vertical bar shows proposed present mantle discharge rate of basalt from the Hawaii plume, based on sustained eruptions by Kilauea in the interval 1952–1971 (ref. 15).

crease continentality and increased volcanism and circulation increase atmospheric dust. Other effects, such as altered oceanic circulation due to altered ocean basin geometries, and eustatic sea level changes due to geoid changes (R. W. Fairbridge, unpublished) attending mass redistribution by plume convection, might affect global climate, but no systematic correlation would be expected. The second mechanism seems the most plausible at this time, although some of the others could make significant contributions.

The correlation between high plume discharge and high reversal frequency is not overly convincing (Fig. 5), but if plumes do originate near the core-mantle boundary, changing plume convection might accompany changes in core convection because of changed boundary conditions at the core-mantle boundary. Exactly why higher discharge should accompany more frequent reversals is a matter for theoretical speculation. Possibly the increased volcanic and plate tectonic violence during high discharge intervals provides more reversal-triggering events of non-core origin, in addition to those normally supplied by the core itself³⁶. Some degree of coupling between plate motion and reversal frequency is suggested by the observation that the greatest change in the motion of the Pacific and probably other plates occurred about 40 m.y. BP, during the time of the highest peak in reversal frequency (Figs. 4 and 5).

The number of conceivable ways high plume discharge could decrease sedimentation rate is very large; detailed analysis is beyond the scope of this article. Different calcium carbonate preservation is not the explanation; carbonate content of Atlantic and Pacific sediments seems to have varied 180° out of phase in the past 70 m.y., while the late Cretaceous/early Tertiary sedimentation minimum affected much of the world ocean³¹. Nor can bottom currents be invoked to explain this world-wide minimum, for where could the sediments then have been swept? Greater ocean floor height expected at least near plumes would produce the opposite effect—higher sedimentation rates. Detritus from oceanic volcanoes and continents would tend to increase sedimentation rates during periods of high plume discharge and vigorous plate tectonics. But in areas of predominantly pelagic sedimentation this increase would probably be entirely masked by variations in biogenic productivity, preservation, and bottom current effects. How then can the coincidence between discharge highs and sedimentation lows be explained? One promising avenue, it seems to me, is to rehabilitate Cloud's argument³⁷ that the faunal community, and particularly marine or marsh-land organisms, are sensitive to variations in trace-metal concentrations in their aquatic milieu and that environmentally induced variations in these elements may explain the great extinction crises that have punctuated faunal evolution since Cambrian times^{38,39}. The latest major extinction crisis occurred at the Cretaceous/Tertiary boundary, variously dated at 63 to 70 m.y. BP. Within dating uncertainty, this crisis, which is known to have eliminated numerous genera of such marine organisms as foraminifera and presumably decimated many others, coincides with the great plume-volcanic outburst that produced the Deccan Traps²⁸, the early Tertiary Brito-Arctic flood basalts^{18,19,23,27,42} and the unique volcanogenic layer in the central North Atlantic³⁰. It therefore seems admissible that the low pelagic sedimentation rates of the late Cretaceous/early Tertiary reflect greatly reduced primary biologic productivity in the world ocean, and that volcanogenic trace metals³⁷, delivered in toxic doses by increased plume-generated volcanism or volcanic exhalations at this time, were responsible for inhibiting productivity and driving diverse genera to outright extinction. (Although this is not a unique explanation, the unfossiliferous nature of the Atlantic volcanogenic layer³⁰ then makes sense.) Sporadic cold pulses, created by volcanic dust canopies in the stratosphere, and lasting years or decades, may have been an additional factor. Because both the Permian-Carboniferous and Plio-Pleistocene glaciations were not associated with great extinctions^{38,39}, it is difficult to believe that continentality and cold alone adequately explain the faunal

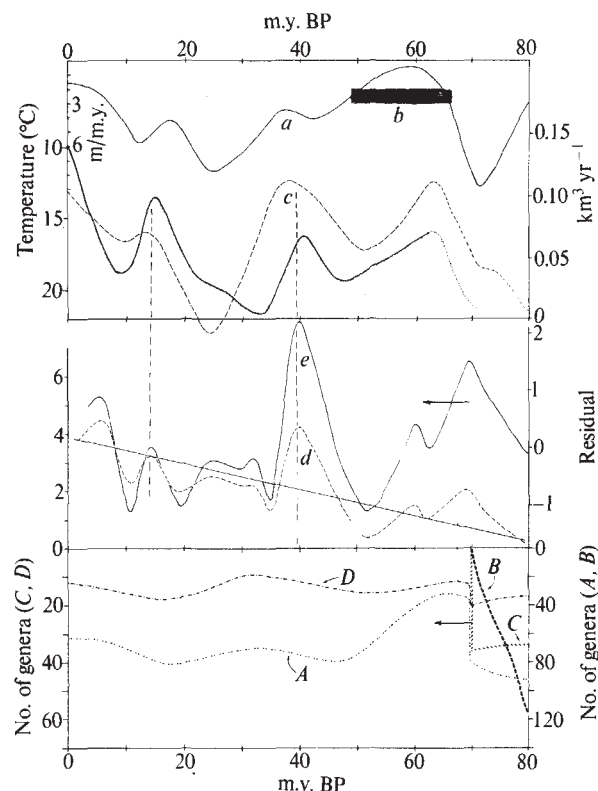


Fig. 5 Comparison of Hawaii discharge curve (bold line, derived from Fig. 4), assumed to mirror total world hot spot curve as argued in text, with: *a*, Pacific equatorial sedimentation rate³¹; *b*, central Atlantic volcanogenic layer and sedimentation minimum (bar)³⁰; *c*, palaeotemperature^{32,33}; *d*, magnetic reversal frequency, smoothed by averaging over 5 m.y. (ref. 34); *e*, residual smoothed reversal frequency, obtained by removing linear increase with time; and diversity of genera³⁵ for corals (*A*), dinosaurs (*B*), globigerine foraminifera (*C*), and rudists (*D*). Note the suggestion of correlation between higher plume discharge, higher residual reversal frequency, lower palaeotemperature, and decrease in diversity of genera. Palaeotemperature and diversity of genera are plotted decreasing upward to facilitate comparison. Arrows show permissible leftward shift in age of oldest third of reversal history⁴ and age of Cretaceous/Tertiary boundary.

crises; evidently large tropical habitats survived even during severe glacial minima. The same objection can be made to Newell's hypothesis³⁵ that lowered sea levels destroyed habitats: it seems unlikely that such habitat destruction could have been thorough enough. The trace element hypothesis³⁷, however, provides a geologically rapid, global mechanism.

Another credibly global and thorough agent for extinction involves reversals of the geomagnetic field⁴⁰, although they are unlikely to have been the primary cause. Reversals were indeed rare or absent in the mid-Cretaceous but became more frequent in the late Cretaceous. But about 10 reversals predated the faunal crisis, and extinctions were not more frequent around the 40 m.y. BP reversal maximum (Fig. 5). Furthermore, some earlier faunal crises were actually associated with decreasing reversal frequency⁹.

To be credible, the plume/trace metal hypothesis requires that peaked plume-volcanic discharge actually correlates with extinctions in time, not only for the late Cretaceous/early Tertiary but also for earlier extinction crises. Fortunately it seems that both the faunal crises³⁸ and the volcanic episodes^{18,25,28} may have taken only about 5 m.y. Coincidence between such brief events, if demonstrated, would be convincing. As plotted in Fig. 5, both the faunal crisis and the reversal maximum appear to precede the volcanic peak by about 5 or 10 m.y. But the reversal time scale³⁴ seems to be too old by this amount⁴¹, and the Cretaceous/Tertiary boundary could equally well be at 65 or even 63 m.y. BP (refs. 38, 39).

At the same time, argon loss suggests that many of the volcanics could be a little older than indicated by K-Ar dating⁴². Thus, present uncertainty makes exact synchronicity attractive but unproven; still more precise dating is obviously needed. The volcanic phase almost certainly continued into the lower Tertiary; the extinct genera^{38,39} almost certainly did not. It follows that the extinctions could only relate to the beginning, or possibly the climax, of the volcanic episode.

Evidence for plume discharge fluctuations before the late Cretaceous is more sketchy; nevertheless, some patterns emerge⁹. Frequency of isotope dates on non-orogenic igneous deposits (especially flood basalts and dolerite dike swarms) suggests discharge peaks about 110–130 m.y. BP, 160–170 m.y. BP, 190–200 m.y. BP and 220–240 m.y. BP. (The latest event is known to be represented only by the volcanics of the Newark Series in eastern North America.) Interestingly, these earlier plume events, as well as the 70–50 m.y. BP outburst previously discussed, fall close to geologic period boundaries, and period boundaries are marked (partly by definition) by faunal crises of greater or lesser extent^{38,39}. Reversal frequency increased about 230 and 70 m.y. BP corresponding to major extinction crises^{41,43}; the frequency decreased about 160–170 m.y. BP (ref. 44) and 110–130 m.y. BP (refs. 9, 44). Thus reversal frequency appears to change during active plume convection, and some sort of “biomagnetic” influence seems to be supported by the coincidence of two increases in reversal frequency with the most spectacular Permo-Triassic and late Cretaceous extinctions⁴⁰. Although handicapped by the lack of direct evidence for exceptional global volcanism at the Permo-Triassic boundary (partly because no seafloor of this age survives, of course) the trace-metal hypothesis is in competition to explain the Permo-Triassic extinction: the Kupferschiefer formation^{38,39} blanketed late Permian seafloors from Germany to the Urals with unusually metal-rich shales of thickness and areal extent comparable to the metal-rich late Cretaceous/Early Tertiary formation entombed in the western central Atlantic³⁰. Although the Kupferschiefer was deposited in shallow water rather than deep, this distinction seems irrelevant. Abyssal sediments of Kupferschiefer age, surviving in exposed subduction zones, ought to reveal exceptional volcanic activity (possibly including hydrothermal exhalations) if the hypothesis has general merit.

The suggested correlation between the last four period boundaries and heightened plume volcanism is strengthened by the observation that, with a few exceptions, period boundaries are marked by emergence and reduced sea levels in many areas³⁵. Present hot spots are associated with regional topographic bulges^{5–7} with wavelengths of 1,000 km or more; it is reasonable that most or even all epeirogenic uplifts—whether associated with volcanism or not—reflect the existence of hot, low density regions in the asthenosphere, derived from plume convection. At least the plume hypothesis readily accounts for such uplifts. It is therefore tempting to speculate that not only the last few but all ten period boundaries represent times of accelerated plume convection. (Some period boundaries are also marked by intensive orogenies, but many are not. Neither a correlation between extinctions and orogeny nor a globally synchronous orogenic periodicity has found general acceptance^{35,38,39}. This is not surprising: present theory relates orogeny to plate collisions involving one or two continents; such collisions depend in part on the accidents of plate geometry, and two continents could well collide during a time of reduced plume convection and subdued plate tectonics.)

If plume convection is the primary mechanism reshaping the Earth's crust, it seems unlikely to me that the environmental disturbances registered by the biosphere in the form of faunal crises do not reflect the activity of plumes. This general conclusion does not depend on the exact causal pathways leading from mantle convection to faunal extinction⁹. The period lengths themselves are surprisingly uniform⁴⁵, with a mean near 50 or 60 m.y.; it is tempting to view this figure

as the length of a basic “Earth spot cycle” characterizing mantle convection at least since the Precambrian. Present dating uncertainties make a still greater uniformity admissible. The more recent, better dated boundaries are closer to the mean. Interestingly, the mean period length and the deduced late Tertiary increase in plume discharge (Figs. 2–5) suggest that the Earth is approaching another “period boundary”. Such attendant late Tertiary phenomena as epeirogenic uplifts—setting the stage for continental glaciations, vigorous seafloor spreading, plate tectonics, and enhanced volcanism—are beyond dispute. In addition to the environmental havoc that is man's handiwork on this planet, a “natural” faunal crisis might be, geologically speaking, just around the corner.

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LETTERS TO NATURE

PHYSICAL SCIENCES

Terrestrial Heat Flow, the Neutrino Problem, and a Possible Energy Source in the Core

DETAILED measurements in a single borehole have revealed a real variation of heat flow with depth¹. To determine the origin of these variations we determined a detailed profile of the U, Th and K concentrations in the rocks through which the borehole passed to estimate the heat production values. A similarity between the heat flow and heat production profiles was indeed found² but, using the presently accepted values for the heat production constants of the common radio elements, the magnitude of the heat production variations was not sufficient to explain the magnitude of the heat flow variations.

In addition to experimental errors a possible source of error could lie in the heat production constants used, the presently accepted values being based upon work by Birch³. Using the most up to date data available, we have therefore recalculated the heat production constants for those radio elements which might make a significant contribution to the observed heat flow. There are many long lived radioisotopes which produce energetic decays, but not all of them are in sufficient concentrations in the crust to be of significance. In this context we have used the empirical relation

$$\lambda P > 10^{-17} \text{ s}^{-1}$$

where λ is the decay constant in s^{-1} and P the commonly accepted value of the isotopic abundance^{4,5} in p.p.m., in granitic rock. This relation clearly includes ^{40}K , ^{87}Rb , ^{232}Th and ^{238}U , and establishes ^{235}U , and possibly ^{147}Sm , as borderline cases. Although many individual elements are excluded by a wide margin, one could question whether the integrated effects of those presently excluded are significant.

Initially, our approach closely follows that of Birch who used two independent methods. The first summed the kinetic energy of the particles emitted, and of the recoil nucleus and the energy of the electromagnetic radiations involved in the decay. The second used the mass difference between the parent and daughter nuclides. There are dangers in using either method because the first requires an extensive knowledge of the radioactive decay processes while the second requires very accurate figures for masses of the nuclides involved. The data we used were taken from Lederer *et al.*⁶ for the first method, and from Heath⁶ for the second method. Only those particles or radiations which have an "absolute intensity" greater than 1% are included in the calculation where "absolute intensity" is defined as the number per hundred disintegrations of the parent nuclide.

Computations by the first method are protracted but fairly straightforward for ^{232}Th , ^{235}U and ^{238}U because the decay schemes are well known; checking the values so obtained by the second method is also fairly straightforward because the mass differences of the nuclides involved are large enough to give reliable equivalent energies. Our results for both methods are given in Table 1, together with the earlier values given by Birch, in terms of the "at site" energy release.

There is better agreement between the values obtained by the two methods than reported by Birch, but because we have omitted from consideration those emissions with an absolute intensity of less than 1% we adopt the values obtained from the differences in atomic mass as the more reliable ones.

The differences between our values and those of Birch are minor; if we follow Birch further and allow that two-thirds of the β decay energy escapes from the Earth as neutrino and antineutrino energy (because we are primarily interested in energy relations we will not further differentiate between neutrinos and antineutrinos), and assume that the remaining energy released is absorbed near the production site, the similarity between the last two columns of Table 1 is also explained.

Table 1 Energy Released and Absorbed "at Site"

Isotope	Total energy released (MeV/atom) Birch ³		Present study		Energy absorbed (MeV/atom)	
	Decay energy	Mass difference	Decay energy	Mass difference	Birch	Present study
1	2	3	4	5	6	7
^{238}U	51.6	52.2	51.4	51.7	47.4	47.7
^{235}U	46.2	47.2	45.8	45.9	45.2	43.9
^{232}Th	42.6	44.4	42.0	42.8	39.8	40.5
^{147}Sm			2.3	2.2		2.3
^{87}Rb			0.27		0.04	0.09
^{40}K			1.40		0.71	0.11

The calculations for ^{40}K are more complicated and less reliable because there are four different decay modes with uncertain values of the various decay constants and energies of radiations. Because there is no significant α energy in any of the four decay schemes a much higher proportion of the energy released is imparted to the neutrino. There is little point in attempting to check the figures by the second method because the mass difference is about 10^{-4} atomic mass units and the data available to date are not accurate enough. Similar restrictions apply to ^{87}Rb and ^{147}Sm .

So far we have followed the approach of Birch fairly closely and, in spite of more reliable data, it is hardly surprising that the resultant figures are in close agreement with his. Recently, however, doubt has been cast on the assumption that the neutrino is necessarily stable.

The solar neutrino problem is probably well enough known but, briefly, it involves a serious discrepancy between the observed and predicted neutrino fluxes of solar origin. To account for this Bahcall *et al.*^{7,8} have suggested that although the neutrino is known to be stable over path lengths of the order of 10^4 cm it may be unstable over path lengths greater than this, the actual path lengths depending of course on the composition of the material being traversed as well as the original energy, but with an upper limit of the order of 10^{13} cm. If the path length is near the lower limit, and whatever the decay system might be, the products are short range particles; then although the energy from naturally occurring radionuclides imparted to the neutrinos is not immediately absorbed "at site" it will be absorbed within a radius of 100 m

of the energy production site. If most of this energy subsequently appears as heat then we should add back the neutrino energy we originally subtracted on the grounds that their stable path length was far greater than the radius of the Earth. Perhaps, the whole of the energy in the radioactive decay processes going on in the crust would then appear as thermal energy in the crust although not as "at site" thermal energy. Table 2 gives the values of heat production for the significant radio elements both with and without this neutrino energy. For those elements with complex decay schemes the difference is marginally significant whereas for the others, particularly potassium, it is highly significant.

Table 2 Heat Generation in $\mu\text{W kg}^{-1}$

Element	Without neutrino energy		With neutrino energy
	Birch ³	Present study	Present study
U	98 (0.74)	97 (0.73)	106 (0.80)
Th	27 (0.20)	27 (0.20)	28 (0.21)
Sm		0.0475 (358×10^{-6})	0.0475 (358×10^{-6})
Rb	0.0048 (36×10^{-6})	0.0129 (97×10^{-6})	0.0387 (292×10^{-6})
K	0.0036 (27×10^{-6})	0.0034 (26×10^{-6})	0.0068 (51×10^{-6})

(The more familiar figures in $\text{cal yr}^{-1} \text{g}^{-1}$ are given in parentheses.)

Two important effects of this are: first, any correlation between vertical variations of heat flow and heat production would be "smeared" which is an effect we believe we have already noticed²; second, where lateral variations of heat flow occur and can be correlated with heat production the heat flow-heat production plot would produce a line with a slope which is 10 to 20% lower than that obtained using Birch's data. In spite of the advantage to our earlier work of the first effect, it is probably unrealistic to place much faith in accepting the minimum stable path length as being the actual path length. But other intriguing possibilities occur.

As in many cases in which there is controversial evidence involving difference of several orders of magnitude, we take the logarithmic mean between the proposed maximum and minimum path lengths. This implies that the stable path of the neutrino would be approximately 3,000 km in length. Some of the energy from the decay of naturally occurring radionuclides concentrated in the crust would therefore be transmitted a few thousand kilometres and appear as thermal energy in the lower mantle and core and could contribute to the energy required⁹ to drive the dynamo.

This mechanism has the advantage that it may not require the distribution of significant quantities of radionuclides within the core, although it does not rule out the possibility that this also occurs. There would also be a "focusing" effect because half the released neutrinos would be travelling in the direction of decreasing radius and increasing density. This raises the further possibility that if the mean stable path length of the neutrino is only 300 km, instead of the 3,000 km assumed above, much of the energy would be released as heat at a range of depths centred around 300 km; this is, of course, in the region of a well known second order seismic discontinuity. The problem here is that there then should be differences in the depth of the discontinuity beneath continents and oceans. Similarly, the apparent equivalence between continental and oceanic heat flows cannot be completely explained in this manner although it would tend to reduce the magnitude of the problem required to explain this apparent equivalence¹⁰.

Clearly there is a significant amount of energy imparted to neutrinos in the decay of naturally occurring radionuclides. If the neutrino is an unstable particle then there is a distinct possibility that some of the thermal energy in the core and mantle originates from radionuclides in the crust, and it is of

great importance to understanding the thermal history of the Earth that we obtain a much more accurate estimate of the stable path length of neutrinos.

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Jupiter Occultation of Beta Scorpii: Are the Flashes Time-symmetric?

AN outstanding feature of the May 13, 1971, occultation of Beta Scorpii AB and C by Jupiter was the occurrence of numerous light flashes, or "spikes". In a discussion of our observations¹ we noted that at a time resolution $\delta t \sim 1$ s, the pattern of some of the spikes observed during the emersion of β Sco AB seemed vaguely time-symmetric with that seen during immersion. But other portions of the occultation curves seemed to be largely uncorrelated at this δt . Fairall² has since published the results of photographic observations of the bright star event. He suggests that the spikes are on the whole significantly time-symmetric, and infers from this a global stratification of the Jovian atmosphere. We have tested this suggestion on our data, which are more extensive and have an improved δt and photometric accuracy.

Our observations were made with the 152-cm reflector of the Boyden Observatory, RSA. Simultaneous records of the events were obtained at $0.34 \pm 0.02 \mu\text{m}$ (Channel 1), $3934 \pm 12 \text{ \AA}$ (Channel 2), and $6215 \pm 75 \text{ \AA}$ (Channel 3), with $\delta t \leq 0.01$ s. Our discussion here will be restricted to the observations of the Beta AB event.

Immersion and emersion of Beta Scorpii AB as seen in Channel 2 with $\delta t = 1$ s are shown in Fig. 1. Both curves have been normalized, and aligned by eye (with the immersion curve time-reversed). The two curves have a similar appearance, with spikes of comparable amplitude and width. Some of the spikes in Section II of the curves seem to correlate, but very little correlation is evident in Section I.

A quantitative comparison can be made using statistical techniques once the background occultation curve (on which the spikes sit) has been removed, as in Fig. 2, for Section II of Fig. 1. Correlation coefficients³ were computed by sliding the reversed immersion curves along the emersion curves. We have carried out a series of such calculations for $\delta t = 1.0$ and 0.1 s for the following sets of data:

- (A) Emersion (Channel 2) with Immersion (Channel 2), Sections I and II.
- (B) Same as (A), but Section I only.
- (C) Same as (A), but Section II only.
- (D) Emersion (Channel 2) with Emersion (Channel 1).

Case D is a test of our procedures because the same spikes occur in all three channels. The results of the calculations are

Fig. 1 Comparison of the emersion (—) and immersion (---) occultation curves of β Sco AB ($\lambda=0.39 \mu\text{m}$, $\delta t \sim 1 \text{ s}$). Both curves have been normalized to unity, and the zero and unit levels indicated. The immersion curve has been time-reversed, and the alignment was made by eye.

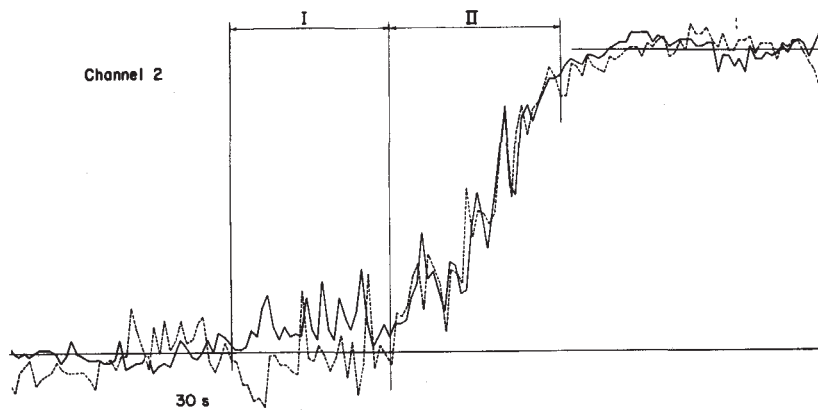


Table 1 Correlation Coefficients (Summary)

Case	No. of data points	Time resolution (s)	Correlation coefficient (maximum)	
			Real data	Nonsense data
A	60	1.0	0.26	0.13–0.32
B	30		0.34	0.34–0.52
C	30		0.44	0.34–0.52
D	60		0.82	0.13–0.32
A	600	0.1	0.19	0.13–0.22
B	300		0.28	0.29–0.35
C	300		0.30	0.29–0.35
D	600		0.82	0.13–0.22

summarized in Table 1. As expected, calculation (D) yields a high correlation coefficient for both time resolutions.

None of the other cases yields so large a correlation coefficient, and we conclude that the pattern of spikes in the immersion and emersion curves cannot be highly correlated. To assess the actual degree of correlation we have also calculated correlation coefficients for sets of nonsense data manufactured from the real data. For example, we have compared Section I (Emersion) with Section I (Immersion), without time reversing the latter. Another such calculation compared Section II (Emersion) with Section I (Immersion), again without time reversing the latter. We calculated correlation coefficients for about eight sets of nonsense data (Table 1). Both for $\delta t=1 \text{ s}$ and $\delta t=0.1 \text{ s}$, the correlation coefficients for the real data in cases (A), (B), and (C) fall within the range of those given by the nonsense data. We conclude that the spike patterns in our occultation curves show no significant correlation between immersion and emersion.

The physical nature of spikes is not well understood, although they are commonly attributed to density fluctuations^{4,5}. Such

fluctuations may be a characteristic feature of the upper atmospheres of Jovian planets, because spikes have also been observed during an occultation by Neptune⁴. We find no evidence in our data that the pattern of fluctuations is planet-wide in the case of Jupiter, and it is therefore improper to speak of global layering. The pattern of density fluctuations may be grossly similar at various locales on Jupiter, but individual layers do not seem to be planet-wide.

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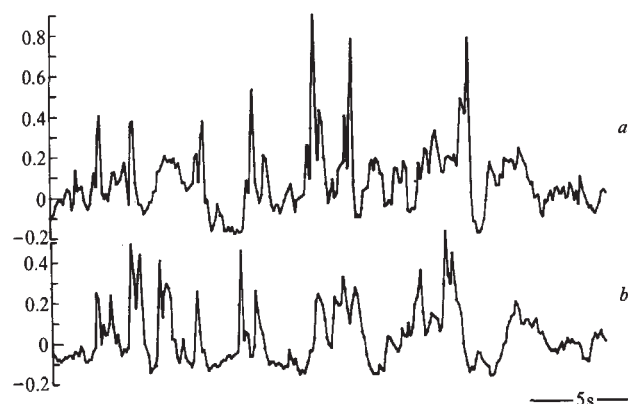


Fig. 2 As Fig. 1, but for Section II only, with the background occultation curve removed. Time resolution: 0.1 s. The alignment is based on correlation coefficient calculations. a, Immersion (reversed); b, emersion.

Radon-222, CO, CH₄ and Continental Dust over the Greenland and Norwegian Seas

ATMOSPHERIC processes in the Arctic and Antarctic areas have received increased attention because of their global importance^{1–3}. We report here measurements of ²²²Rn, CO, CH₄ and continental dust in air over the open Norwegian and Greenland Seas and in the ice-covered Greenland Sea during August 1971.

The experiment was undertaken to investigate the relationships between these parameters and air mass trajectories. Radon (²²²Rn)—a radioactive rare gas emanating essentially from large continental land areas—and dust show the presence of a continental component in a marine air mass can be detected. ²²²Rn does not react chemically; its lifetime is limited only by its radioactive half-life of 3.82 day. Dust is also picked up over land areas, but may be removed by coagulation and fallout or washed out by rain or fog, although comparison of radon level and dust data may be helpful in

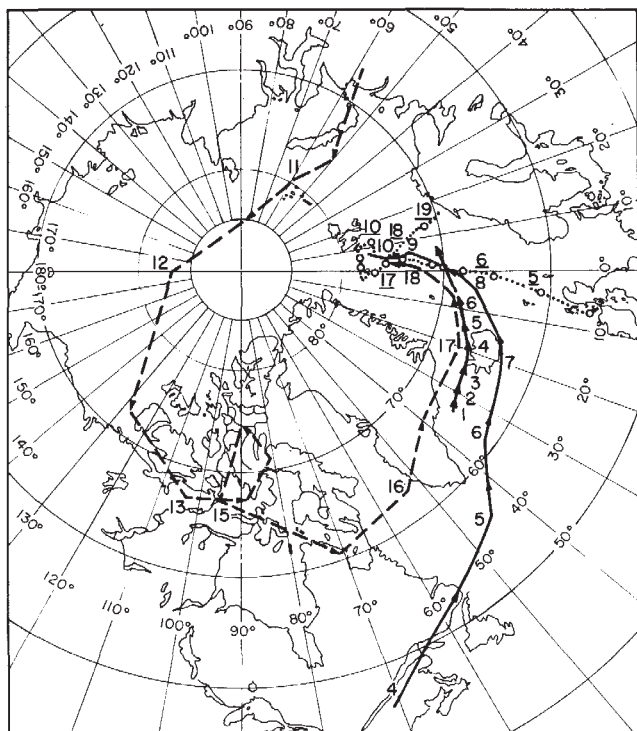


Fig. 1 Air mass trajectories and cruise track of USNS Mizar., Track of USNS Mizar; other tracks show typical air mass trajectories. Arrow heads represent positions of the air masses at 2400 GMT on the dates indicated by numerals. Circles represent positions of the USNS Mizar at 0000 GMT on the dates indicated by underscored numerals. The USNS Mizar was in the pack ice from August 11 to August 15 and daily positions are not specified.

tracing the trajectory and mixing of air masses⁴; for example, radon measurements⁵ show that monsoon air masses contain continental air. Prospero and Carlson⁶ compared radon measurements with visual haze observations over the equatorial Atlantic and suggested the use of radon as a tracer for African air parcels.

Robinson and Robbins⁷ reported an average value of 0.10 p.p.m. for CO in clean Arctic air over the northern Greenland ice cap. They identified a continental air mass by the changes in CO concentration and suggested that CO could be used as a tracer for polluted air masses.

Data on radon, dust, carbon monoxide and methane were obtained during an Arctic cruise of the USNS Mizar in August 1971 following the course shown in Fig. 1. Throughout the voyage, measurements were made of radon, CO, and CH₄. Two samples of dust (particle diameter >2 µm) and three samples of fog water were collected and concentrations of these atmospheric constituents analysed and correlated with the history of the air masses present.

Radon reaches equilibrium with its decay products through ²¹⁴Pb within a few hours. These quickly attach to aerosol particles and can easily be collected on filters; thus radon concentrations can be determined from the β-radioactivity of ²¹⁴Pb and ²¹⁴Bi collected on filter paper, provided care is taken to ensure reasonable equilibrium. Beta activity was measured with thin plastic scintillators mounted on photomultiplier tubes. Air was drawn through 10.2 cm diameter filters (effective diameter 8.5 cm) at the rate of 1.6 m³ min⁻¹; sampling time was usually 20 min. Samples were collected 8 m above the surface of the water at the bow of the ship. Radon decay products can be removed from the atmosphere by rain or fog, but build-up of the isotopes of interest is such that, within an hour after the end of rain or fog, the maximum error caused by assuming an equilibrium condition is less than 25%.

Nylon mesh sails (or kites) were used for dust collection. Approximately 0.5 × 10⁶ to 1 × 10⁶ m³ of air passes through the sails in 24 h if the relative wind velocity is 25 knot. Dust particles were washed from the sail with distilled water and separated with a centrifuge, and those larger than 2 µm diameter were collected with 50% efficiency.

Table 1 Comparison of Dust and Fog Results

	Kite 2 August 7-11	Kite 5 August 17-18	Fog 1 August 15 1845-1915	Fog 2 August 15 2000-2100
Dust	0.005 µg m ⁻³	0.1 µg m ⁻³		
Radon	3 pCi m ⁻³	7.3 pCi m ⁻³	2 pCi m ⁻³	2 pCi m ⁻³

Moisture from fogs was collected with similar sails, which were lowered when there was a possibility of another ship being up wind or when the manoeuvring of the Mizar might result in stack gases being collected. Sails were lowered during rain or fog except while collecting fog water.

CH₄ and CO measurements were taken at intervals of 3 h independent of weather or other activities. Samples were taken through an air inlet on the bow, approximately 8 m above sea level. The gases were concentrated on a cold trap and identified by a flame ionization detector. CO is converted to CH₄ by passing it over reduced nickel in the presence of H₂. Air mass trajectories were established from analysis of twelve-hourly synoptic 500 and 700 mbar charts.

Individual radon measurements (dots) and daily averages (bars) are shown in Fig. 2. No samples were collected during rain or heavy seas that might have washed away sampling equipment. Radon concentrations were high while near land but decreased as we proceeded north into the Norwegian Sea, and remained low from August 7 to August 17. During this period, at least one radon sample was taken each day at a time sufficiently removed from rain or fog for equilibrium to have been established between radon and the beta emitting isotopes ²¹⁴Pb and ²¹⁴Bi.

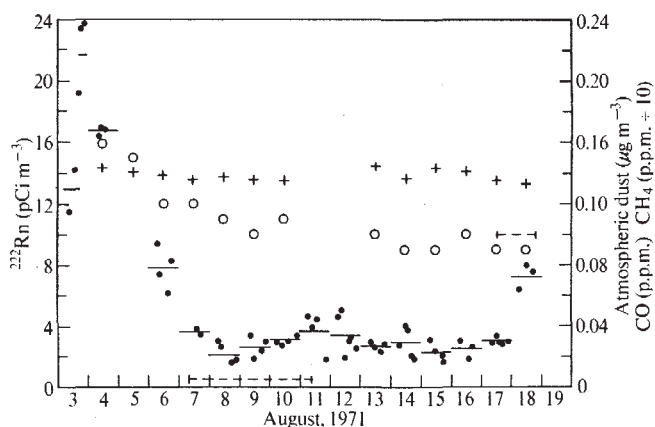


Fig. 2 Atmosphere components measured during August 1971. ●, Individual ²²²Rn level; —, average ²²²Rn level; ---, dust; ○, CO; +, CH₄.

Average daily CO and CH₄ concentrations are also shown in Fig. 2, together with two dust concentrations. From August 4 to August 7 CO concentrations were high (0.17 p.p.m.) but continued to decrease as the ship proceeded away from land masses. CH₄ concentrations averaged 1.40 p.p.m. during this period with slight variation.

From August 7 to August 11 the surface winds were in the west to north quadrant with velocities of 3 to 8 knot. Radon concentrations in the range of 2 to 4 pCi m⁻³ and a low dust concentration of 0.005 µg m⁻³ confirmed that we were in a

clean marine air mass. The air mass over the Mizar on August 7 and 8 (short solid path in Fig. 1) spent several days over the ocean. It was just east of Greenland (65° N, 35° W) on August 1 and reached the Mizar (70° N, 0° W) on August 7. The more rapid movement of the air mass that reached the Mizar on August 11 (long solid path in Fig. 1) explains the slightly higher radon counts observed on August 11 and 12. From August 12 to August 17 the ^{222}Rn concentration remained near 3 pCi m^{-3} while the surface winds began to shift counter-clockwise from northerly to southerly. No dust samples were collected throughout the period from August 11 to August 17. CO concentrations averaged 0.09 p.p.m. and CH_4 concentrations were fairly constant at 1.38 p.p.m. These values agree well with those of Robinson and Robbins⁷ and Cavanagh *et al.*⁸ for Arctic air masses.

On August 18 surface winds shifted to the southeast blowing from the direction of Scandinavia, but the 500 and 700 mbar air mass movements (dashed line in Fig. 1) show that the air mass originated over northern Russia and remained in cold areas as it passed near the north pole, over northern Canadian islands and Southern Greenland, and spent two days over water. The radon level on August 18 almost tripled and a dust sample started late on August 17 showed a concentration of $0.1 \mu\text{g m}^{-3}$, an increase over the previous sample by a factor of 20. The CO and CH_4 concentrations on August 18 (0.09 and 1.33 p.p.m. respectively) were largely unchanged although ^{222}Rn and dust measurements confirm a continental air mass. Robinson and Robbins⁷ reported a large systematic increase in CO (0.05 to 0.65 p.p.m.) over the Greenland ice cap and attributed this to a large air mass from the industrial northeastern United States. We note that the air mass observed between August 11 and August 17 which has passed over the industrial northeastern United States did not show an increase in CO or CH_4 concentrations. The reason for the discrepancy may be attributed to the climatic conditions (that is, inversion layers) in the areas where air masses originate or pass.

Both dust samples collected over the Greenland Sea were of continental origin. The composition was determined by neutron activation analysis and found to be similar to crustal rock⁹. The agreement between continental dust and radon shows that in remote ocean areas radon is a good tracer for continental air masses. The relationship between radon and dust is, however, complex because radon concentration depends on the origin of the air mass and the transit time, while the dust concentration can be greatly reduced by precipitation scavenging in rain or fog.

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Extensive Dust Layer in the Northern Hemisphere

A DUST layer covering much of the northern hemisphere has been observed during the summer of 1972. The altitude of the layer varied between 8 and 9 km with typical submicron particulate concentrations ranging between 1 and 10 per cubic centimetre.

Following the recent, relatively small volcanic eruption of Alaid, $50^{\circ} 51' 5''$ N latitude, $155^{\circ} 34' 0''$ E longitude, on June 19, 1972, in which the ash cloud was reported to be 8 km high (350 mbar) and 5 or 6 km wide¹, an effort was made to assess the downwind effects at a distance of approximately 4,000 miles. The measurements were made at Panama (9° N latitude, 80° W longitude); Palestine, Texas (32° N latitude, 95° W longitude); Laramie, Wyoming (41° N latitude, 106° W longitude); Duluth, Minnesota (47° N latitude, 90° W longitude); and Ft. McMurray, Canada (57° N latitude, 112° W longitude), using a dustsonde described by Rosen².

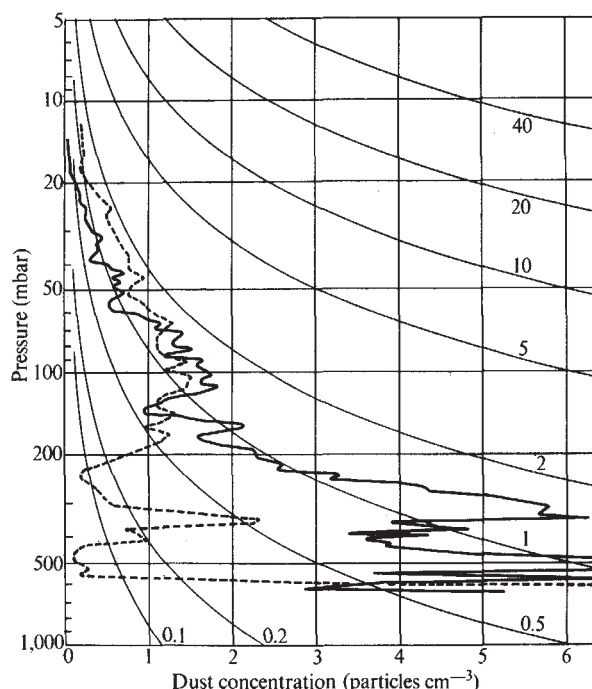


Fig. 1 A comparison of the aerosol soundings made at Laramie, June 28, 1972 (---), and at Ft. McMurray, June 29, 1972 (—). The concentration refers to particle sizes about $0.30 \mu\text{m}$ diameter and larger. The smooth curves are lines of constant mixing ratio.

Fig. 1 is a comparison of the soundings made at Laramie and Ft. McMurray. Both profiles show a layer in the 300 to 350 mbar region (the maximum reported altitude of the volcanic ash cloud). The Texas and Duluth soundings (not shown) also display a layer in this region, but no evidence of one was found in the Panama sounding.

The presence of tropospheric layers in a single sounding is not unusual, but the correspondence in altitude of these layers over a wide range in latitude (32° N– 57° N) is highly suggestive that they are associated with a single event that could be the eruption of Alaid.

Further evidence that these 350 mbar layers are not accidental is shown in Fig. 2. Here, the soundings made at Laramie covering a period of 24 days all show a layer in the 300 to 370 mbar region. The profiles also suggest the presence of other layers but these are not persistent. It seems justifiable to identify tentatively the source of the 300–370 mbar aerosol

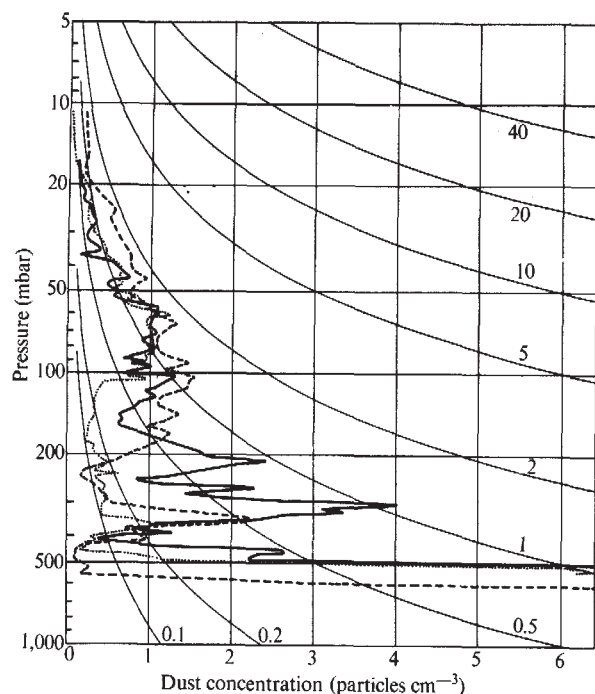


Fig. 2 A comparison of three aerosol soundings made at Laramie, the first on June 26, 1972 (—), the second on June 28, 1972 (---), and the third on July 19, 1972 (...). The concentration refers to particles about 0.30 μm diameter and larger. The smooth curves are lines of constant mixing ratio.

layer covering much of the northern hemisphere with the eruption of Alaid.

Table 1 summarizes the altitude and potential temperature of the dust layer for the individual soundings. The initial latitudinal spreading appears to be at least 1° day^{-1} . In addition, the spreading seems to have been at relatively constant altitude or pressure rather than along lines of constant potential temperature. A systematic decrease in the altitude and concentration of the layer over Laramie is, however, seen in Fig. 2.

Table 1 Dust Layer

Sounding	Altitude (mbar)	Potential temperature
Laramie—June 26, 1972	300	325 K
Laramie—June 28, 1972	350	337 K
Ft. McMurray—June 29, 1972	350	316 K
Duluth—July 4, 1972	360	313 K
Laramie—July 19, 1972	370	325 K
Texas—July 26, 1972	330	338 K

Assuming that the aerosol layer concentrations measured at Palestine, Laramie, Duluth and Ft. McMurray are typical of a global band 40° wide centred at 50° N , then the total aerosol injected by this volcano is a small fraction of the total global aerosol burden in the 300 to 370 mbar region. Thus the dust from this volcano may decay at a rate governed by the horizontal diffusion time rather than the residence time, both of which are relatively short periods in the troposphere. It is important that the observations of small volcanic disturbances be made promptly after the eruption over a rather extensive area.

At present there does not seem to be any change in the 20 km aerosol attributable to the eruption of Alaid. Volcanic gases, however, may have penetrated the tropopause, in which case they would be expected to quickly give rise to very small particles undetectable by the method employed here.

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Geomagnetic Anomalies caused by a Man-made Lake

TECTONOMAGNETISM—the study of magnetic anomalies due to tectonic stresses—is attracting increasing interest as a method of monitoring local changes in Earth stress¹. The most important aspect is the search for the seismomagnetic effect; no convincing observation has yet been reported, although calculations^{2,3} indicate that in many cases it must be of measurable magnitude, and observations of magnetic precursors to creep increments on the San Andreas fault⁴ encourage the view that magnetic effects of earthquakes themselves are observable. Local magnetic changes preceding and accompanying volcanic eruptions in New Zealand are very striking^{5,6} and indicate that substantial stresses are associated with eruptions; a quite different pattern of volcanomagnetic changes in Hawaii (P. D., F. S., Jackson and Field, to be published) suggests that Kilauea volcano is highly fragmented and incapable of supporting a large scale stress pattern. But both volcanic eruptions and the San Andreas creep increments are very imperfectly understood so that a large scale control experiment which demonstrates quantitatively the role of the piezomagnetic effect in producing local magnetic anomalies is very desirable. The requirement is satisfied by the water loads imposed on underlying strata by man-made lakes, which provide large scale Earth stresses of sufficient intensities to produce measurable magnetic anomalies. Recent completion of a new dam at Talbingo in the Snowy Mountains of SE Australia and rapid filling of the reservoir provided an opportunity to observe this effect.

Fig. 1 is a map of the area on which the principal magnetic survey points are marked. Each of these points was carefully established during the first of 15 surveys, so that it could be re-located with a precision of about one inch in subsequent surveys. Altogether eight precisely repeated surveys were made during the twelve months before filling of the lake, three after filling to about 3/4 of total capacity (560,000 acre foot) and four more after virtually complete filling (730,000 acre foot and 480 foot maximum depth). Filling was rapid and the last survey was completed in February 1972, nine months after filling commenced. During each survey, the total field intensity at each point was measured ten times with a portable proton precession magnetometer and the average compared with the simultaneous data from a continuously recording magnetometer at a fixed site 3.5 km north of the dam wall. All readings were then expressed as differences between the survey points and the base station; this process eliminated effects of diurnal, and other, variations in the ambient field.

The pre-fill surveys showed that there was no secular trend in the readings over a period of twelve months before filling began. Comparison with data obtained at the two stages of filling gave the changes listed in Table 1, which are attributed

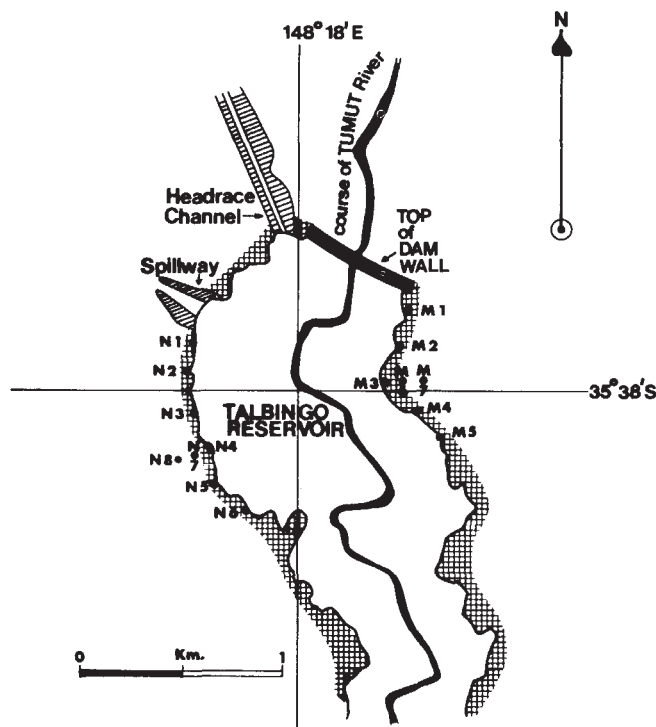


Fig. 1 Map of the reservoir formed by Talbingo dam, with positions of the precisely repeated magnetic survey points. The base station used for standardizing the survey data was 3.5 km north of the dam wall.

to the water load. The standard deviations of the individual difference field readings are 1.7γ . The standard deviations of the changes are taken to be 2.4γ ; that is $\sqrt{2}$ times 1.7γ . The values listed in Table 1 are averages of 30 to 80 data points, so that standard deviations of these mean changes are 0.3 to 0.4γ and there is no doubt about their statistical significance.

Table 1 Changes in Total Field Intensity at the Survey Points marked in Fig. 1

Survey point	Change at three-quarters full (γ)	Change at complete fill (γ)
N1	-5.6	+2.2
N2	-3.3	-5.4
N3	-5.0	-7.4
N4	-1.9	-5.9
N5	-2.0	-4.0
N6	-5.7	-7.9
N7	-5.2	-8.0
N8	-2.2	-4.8
M1	-3.6	-3.2
M2	-4.1	-2.8
M3	-3.8	*
M4	-5.6	-5.6
M5	-3.2	†
M6	-1.0	-4.0
M7	-1.6	-5.3

* Survey point affected by a steel surveyor's spike.

† Point under water.

Standard deviations of individual difference field readings (survey point minus base station) were about 1.7γ and 30 to 80 such readings for each survey point were averaged at each stage; pre-fill, three-quarters and completely filled. The values listed are differences between these averages.

The terrain of the Talbingo area is rough and the geological structure complicated. Detailed geological investigations were confined to the close vicinity of the dam site, so that precise calculations of the anticipated piezomagnetic anomaly field are out of the question. We have computed anomaly fields for several geometrically simple lakes of maximum depth 120 m (the maximum depth in the cross-section between our survey points) assuming the rocks to be magnetically and

elastically uniform. For this purpose the Boussinesq point force equations were integrated over the area of contact of the lakes (with force proportional to depth of water) to obtain the stress field at all points in the medium and the magnetic anomaly field was calculated by the method of Stacey, Barr and Robson⁷, assuming a stress sensitivity of rock magnetization of $2 \times 10^{-4} \text{ bar}^{-1}$ (ref. 8). Although the computed anomaly fields were of different forms, they all showed generally similar negative anomalies, but for maximum changes of about -7γ , intensities of total (induced plus remanent) magnetization of about $5 \times 10^{-3} \text{ e.m.u.}$ were required in the underlying rocks. This is substantially higher than the magnetizations of the predominantly rhyolitic rocks in the immediate vicinity of the dam. Samples of bore core from a 104 m drill hole at the dam site had variable total magnetizations up to a maximum of $1 \times 10^{-3} \text{ e.m.u.}$ The dam magnetic effect implies therefore that the geological structure at greater depths includes more strongly magnetic rock. A stratigraphic section of the dam site⁹ indicates a layer of dolerite at almost 2,000 foot and andesitic basalt at greater depth.

Gough and Gough¹⁰ found with Lake Kariba that earthquakes triggered either by water load or by increased pore pressure in underlying strata significantly redistributed the regional stresses. Local earthquakes recorded during the period of our measurements by seismometers in the Talbingo area (Cleary and Timmel, personal communication) were all of magnitudes less than 2.4. Over the volume of rock effectively sampled by the magnetic surveys, we calculate that the total stress change associated with these earthquakes was smaller by a factor exceeding 10 than that due to the water load. Thus the static load appears to have been almost entirely responsible for the magnetic changes. Movements of survey points by ground deformation, due to loading are also insignificant, although we did take the precaution of measuring the field gradient at each of the survey points. Gradients ranged from 0 to $30 \gamma \text{ m}^{-1}$. Highly localized gradients are due to inhomogeneities of rock magnetization in the immediate vicinity of each point so that the inhomogeneities themselves moved with the survey points and so could not have produced the observed effects. In any case the gradients were variously oriented, so that movement would not have produced an apparent consistently negative anomaly, as do our data.

The dam magnetic effect itself is of limited interest. The real significance of these observations is that they demonstrate that the piezomagnetic effect is observable on a tectonic scale and thus give observational support to the expectation that the seismomagnetic effect is observable and may be the basis of an earthquake prediction technique.

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Combustion Control by Novel Catalytic Means

CONTRARY to the generally held view, we have discovered that flames can be strongly catalysed, provided that the catalysts are introduced in specific flame zones. This markedly reduces the formation of carbon and of nitrogen oxides, both potential or actual air pollutants. Flame catalysis affords a rather simple and inexpensive method for their control, and the catalyst itself does not pollute the atmosphere since it is not introduced as an additive with the fuel-air stream to discharge with the flue gas, but is held in the flame.

Whilst carbon formation as smoke is undesirable, there are applications where a greater production of carbon is required, e.g., in the manufacture of carbon black and for flames with poor emissivity. We find that catalysts can promote carbon formation besides inhibiting it.

Inhibition of carbon formation occurs when catalysts—metals and metallic compounds—are placed in the primary reaction zone of the flame, and promotion occurs when they are inserted in the pyrolysis zone. Nitrogen oxides are reduced when catalysts are introduced in the zone immediately downstream of the primary reaction zone, near the hottest part of the flame. These effects are not restricted to a particular type of flame, whether premixed or diffusion, and are observed with every type of hydrocarbon fuel. Our principal observations, and conclusions, are discussed below: detailed results will be published elsewhere.

The influence of catalyst position in the flame is illustrated in Fig. 1. Catalysts were introduced as thin coatings on 0.1–0.5 mm thick quartz fibre loops and also, where possible, as fine metal wire loops; these had virtually no effect on flame aerodynamics. A marked antismoke effect resulted from their insertion in the primary reaction zone. With a barium oxide coated probe, at 1 cm above the burner, the effect was so pronounced that 36% more fuel could be burnt smoke-free. Downstream at the start of the pyrolysis zone, the probe caused an almost equally marked promotion of carbon formation. Further downstream this effect gradually diminished. The magnitude of the "prosmoke" effect was also measured by collecting and weighing the carbon formed, and these results agreed with those in Fig. 1. Besides increasing the amount of carbon, catalysts increased its particle size.

A wide variety of catalysts were studied, and they all exercised antismoke and prosmoke effects similar to those illustrated for BaO; the efficiency, however, varied considerably. Differences for a number of alkalis and alkaline earths are shown in Fig. 2. Both the antismoke and prosmoke activities strikingly correlate with the ionization potentials. A similar correlation was observed for effects of mixtures of catalysts: In agreement with the known synergistic increase in flame ions for mixtures of

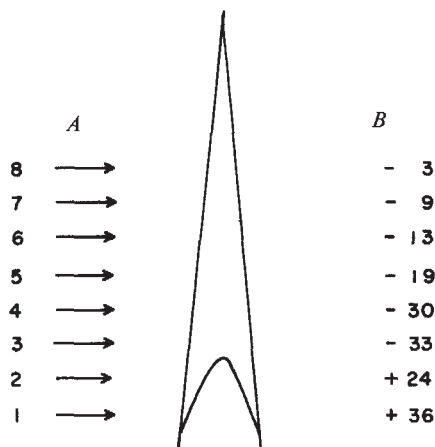


Fig. 1 Influence of position of barium oxide coated quartz probe on carbon formation in ethylene flame. A, Probe height above burner (cm). B, Change in smoke-limited fuel flow (%): negative values, prosmoke; positive values, antismoke.

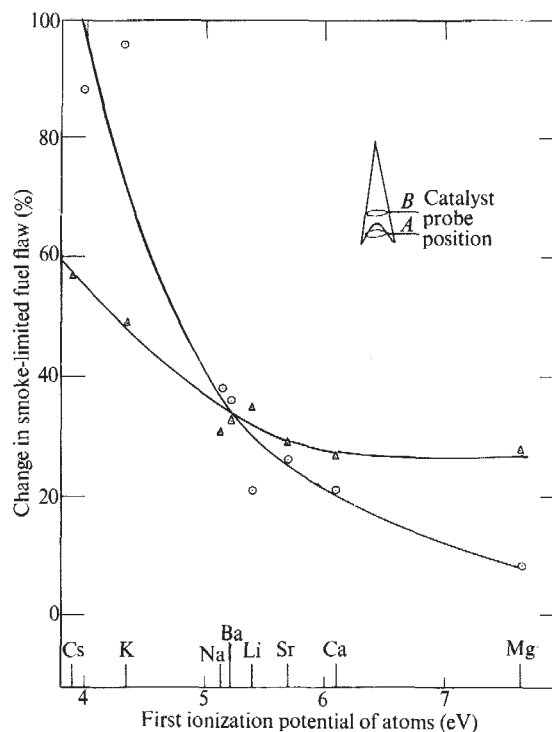


Fig. 2 Relative effectiveness of alkalis and alkaline earths in ethylene flame. $\circ$, Increase in fuel flow with catalyst at A (antismoke effect). Δ , Decrease in fuel flow with catalyst at B (prosmoke effect).

sodium and strontium¹, a synergistic increase in both the anti-smoke and prosmoke effects was observed.

The observed correlation between catalytic activity and ionization tendency suggests that the catalysts function largely through their influence on flame ionization. This also leads to the important inference that flame ions play a significant role in carbon formation; such a role for flame ions has not hitherto been generally accepted².

The observed effects cannot be due to a change in the aerodynamics or temperature of the flame. Catalysts hardly affected flame aerodynamics, and their effect on flame temperature was too small to account for the catalytic effects. In the pyrolysis zone, in the close vicinity of the probe, the temperature drop was only 50° C. Upstream, in the primary reaction zone, the temperature drop, though greater, was still only about 130° C.

We suggest that the antismoke effect of catalysts is due largely to the destruction of carbon-bearing flame ions by electrons released by the catalysts. Our mass spectrometric analyses of flame ions, sampled directly from flames, show that catalysts reduce the natural flame ions and that this reduction is proportional to the activities of the catalysts.

We ascribe the prosmoke effect in the pyrolysis zone to the electrons from catalysts reducing the positive charge on some of the carbon particles. All carbon particles are positively charged from their inception^{3,4}, and a charge reduction will reduce repulsion between them and promote their growth and agglomeration to large and less readily oxidizable particles. Our studies show that the particle size is increased by catalysts.

Nitrogen oxides are markedly reduced when catalysts are placed immediately downstream of the primary reaction zone, near the hottest part of the flame. This position, according to our detailed studies, is where normally the rate of formation of nitrogen oxides begins to rise most sharply. Catalysts differ in their effectiveness. A few typical results, showing percentage peak reduction in nitrogen oxides, are: K, 33; Ba, 28; Sr, 35; Al, 21; Zr, 16; Cr, 25; Mn, 28; Co, 43.

The mechanism of catalytic action on nitrogen oxides must be different from that on carbon: no correlation is observed between the magnitudes of catalytic activities and the ionization tendencies of the catalysts. In broad terms, there can be little doubt that catalysts function by recombining and destroying

atomic and free radical species which are directly and indirectly responsible for the formation of nitrogen oxides. Detailed investigations of the mechanism of catalytic action are in progress.

Flames in several commercial firing systems examined have also been found to respond to catalysts. Thus carbon formation can be inhibited or promoted, and nitrogen oxides reduced, by suitably positioning catalysts in the flames. The magnitude of these effects, however, is somewhat less than in the laboratory flames. We are currently developing suitable catalyst systems for field applications.

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BIOLOGICAL SCIENCES

Transmission of Kuru from Man to Rhesus Monkey (*Macaca mulatta*) 8½ Years after Inoculation

NEUROLOGICAL disease has appeared in a female rhesus monkey (*Macaca mulatta*) following an asymptomatic incubation period of 8 years and 5 months after inoculation intracerebrally (i.c.) and intravenously (i.v.) with a 10% suspension of brain tissue from a human kuru patient. Clinical signs were remarkably similar to those observed in patients naturally affected with kuru and in sub-human primates in which the disease has been experimentally induced by inoculation of the kuru virus. Histopathological examination of the brain of this rhesus monkey has confirmed this first successful transmission of kuru to an old-world monkey. Previously the chimpanzee and four species of new-world monkeys (spider, *Ateles*; capuchin, *Cebus*; squirrel, *Saimiri*; and woolly, *Lagothrix*) have been found to be susceptible to kuru.

In August 1963, one chimpanzee (A2), 5 rhesus, 4 cynomolgus and 3 African green monkeys were inoculated i.c. (0.2 ml.) and i.v. (0.3 ml.) with a 10% suspension of brain tissue from kuru patient Enage. Thirty months after inoculation chimpanzee A2 developed experimental kuru and was killed in the terminal stages of disease 4 months after onset. During subsequent months (between 1 month and 62 months after inoculation) 1 rhesus (16L), the 4 cynomolgus and the 3 African green monkeys died of intercurrent infections without signs of neurological disease; histological examinations by light and electron microscopy revealed no pathological lesions in the central nervous system. The remaining 4 rhesus monkeys remained clinically well until January 1972 (101 months after inoculation) when one (11L) was noted to have occasional tremors and locomotor ataxia. The animal climbed with reluctance and caution and became withdrawn and docile. Her hair coat became rough and there was piloerection over the entire body. Neurological signs became progressively worse and one and a half months after onset she developed clonic jerks of all four limbs and trunk and almost continuous coarse generalized tremors. Two and a half months after onset, although alert to her surroundings, she lay down on her side and had to be fed by hand. She was killed at this advanced stage of disease. At no time during clinical illness was there

any fever nor were significant changes noted in haematological and serum chemistry values.

At necropsy the animal was thin with scant subcutaneous and omental fat. The brain was firm and, on cutting, a blanching of the grey matter was noted. There were no other gross pathological changes.

Preliminary histological examination of the brain by light and electron microscopy revealed extensive neuropathological lesions restricted to the grey matter. There was moderate to severe status spongiosus of the cerebral cortex and basal ganglia, most severe in the deeper layers of the cortical mantle and less extensive in the dentate nucleus of the cerebellum (Fig. 1). In all areas examined there was marked intraneuronal vacuolation, loss of neurones and astroglial proliferation and hypertrophy.

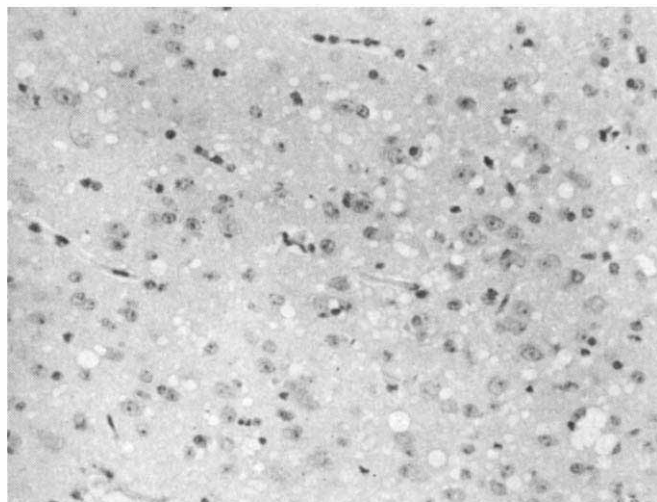


Fig. 1 Status spongiosus in the cerebral cortex of a rhesus monkey (11L) dying with kuru. In the areas of spongiform alteration there is neuronal loss and gliosis. Haematoxylin and eosin stain.

To date Creutzfeldt-Jakob disease, which resembles kuru in both the neuropathology of its cellular lesion and in many properties of its virus, has shown the same species specificity as kuru except for its failure thus far to cause disease in any of the old-world monkeys inoculated. However, none of the animals inoculated with brain suspensions from Creutzfeldt-Jakob disease has yet been observed over asymptomatic incubation periods as long as the 8.5 years required for disease to appear in this kuru-affected rhesus monkey. Rhesus monkeys inoculated with C-J disease virus 42 months ago are still under observation and remain well.

It is of interest that the two spongiform encephalopathies of animals have been transmitted recently to old-world monkeys: scrapie to the cynomolgus monkey¹ and mink encephalopathy to the rhesus monkey². In view of the rapid decrease in the incubation period of kuru in the chimpanzee and some new-world monkeys on serial passage, it seems likely that the host range may be altered by serial passage in different hosts, even perhaps on blind passages in hosts not yet demonstrated to be susceptible.

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Protease Activity establishes the Block against Polyspermy in Sea Urchin Eggs

On fertilization, sea urchin eggs release soluble macromolecules which are referred to collectively as the fertilization product. We have described¹ a trypsin-like activity in the fertilization product which may be involved in the elevation and transformation of the vitelline layer into the fertilization membrane. Soybean trypsin inhibitor (SBTI) abolished the activity of this fertilization protease *in vitro*; if fertilization occurs in SBTI the elevation of the vitelline layer is severely impaired¹.

Hagström reported that fertilization in SBTI increases polyspermy^{2,3}. Here we present evidence that SBTI-induced polyspermy may result from the inhibition of the protease of the fertilization product. Our data show that under normal conditions many sperm attach to an egg by a protease-sensitive bond between the time of insemination and the initiation of the cortical reactions 25 s later (this transitory attachment is not to be confused with polyspermy). The protease activity initiated during the breakdown of the cortical granules cleaves these sperm-egg bonds and all bound sperm except the fertilizing one are detached from the egg surface. Sperm remain attached to eggs fertilized in SBTI and the eggs become highly polyspermic. By contrast, if eggs are first incubated in fertilization product containing fertilization protease activity, sperm do not attach to the eggs and the eggs do not fertilize.

Table 1 shows that increasing the amount of SBTI present during fertilization increases the number of sperm nuclei that penetrate the eggs (details in figure legend). This increased polyspermy does not necessarily result from the failure of portions of the vitelline layer to elevate since a significant increase in polyspermy occurred in low concentrations of SBTI (0.1 mg/ml.) which did not prevent the complete elevation of the vitelline layer. Associated with SBTI-induced polyspermy is the persistent attachment of sperm to the fertilization membrane.

After insemination, a transitory attachment of many sperm to a single egg occurs. This attachment is retained by fixation in 7.5% formaldehyde in seawater. Sperm attachment can be qualitatively estimated by microscopic examination of fixed eggs or quantitatively measured by turbidometric procedures (to be reported elsewhere). When the fertilizing sperm initiates the cortical reaction, the supernumerary sperm detach coincidentally with the elevation of the vitelline layer. This



Fig. 1 Sperm remain attached to eggs fertilized in soybean trypsin inhibitor and elevation of the fertilization membrane is not complete. Normally, the fertilization membrane would appear as a spherical surface completely devoid of sperm. Eggs were fertilized in 1.5 mg SBTI per ml., then fixed for scanning electron microscopy in a 2% solution of glutaraldehyde in seawater for 24 h at room temperature. The fixed eggs were rinsed in distilled water then dehydrated in a graded series of ethanols. The dehydrated eggs were transferred to a 50 : 50 mixture of 'Freon 113' : ethanol and then to pure 'Freon 113'. The eggs were dried by the 'Freon' critical point method¹⁰, mounted on specimen stubs with 'Scotch' double stick tape, dried *in vacuo* for 6 h, rotary coated with about 150 Å of a 60 : 40-gold : palladium mixture and viewed in the 'Stereoscan S4' scanning electron microscope (Cambridge Scientific Instruments, Ltd) of the Scripps Institution of Oceanography Analytical Facility.

Table 1 Induction of Polyspermy by Fertilization Soybean Trypsin Inhibitor

mg SBTI/ml.	Sperm nuclei/egg
0.000	1.54
0.025	1.37
0.050	2.42
0.100	4.54
0.200	5.54
0.400	7.98

Unfertilized *Strongylocentrotus purpuratus* eggs were incubated for 30 min in concentrations of soybean trypsin inhibitor (Sigma) up to 0.8 mg/ml. An equal volume sperm suspension was added to each egg suspension thus diluting the inhibitor concentration in half. Eggs were fixed in 0.2% formalin in seawater at 13 min after insemination and polyspermy determined by counting the sperm nuclei in eggs. At this time the sperm nuclei can be easily distinguished in the egg cytoplasm when the eggs are prepared as described below. The fixed eggs were washed three times by centrifugation and then resuspended in seawater containing 0.1 mg/ml. trypsin (Worthington 2× crystallized) for 20 min at 20° C. This procedure, which removes any sperm still attached to the fertilization membrane, was necessary when eggs were fertilized in the presence of SBTI, since sperm remain bound to the eggs, making it difficult to count the sperm nuclei accurately in the egg cytoplasm. After trypsin treatment, the eggs were transferred to ethanol : acetic acid (3 : 1) for 30 min, cleared in 55% acetic acid and observed with the phase contrast microscope. The number of sperm nuclei in 50 randomly selected eggs was counted and the average number per egg determined. The number of sperm nuclei per egg was proportional to the concentration of soybean trypsin inhibitor.

detachment does not occur when eggs are fertilized in SBTI. Fig. 1 shows the surface of an egg fertilized in SBTI. Many sperm remain attached to the fertilization membrane which has not completely elevated from the surface of the egg. Quantitative studies of the attachment-detachment sequence show that SBTI has no effect on the sperm attachment phase of fertilization; it only prevents detachment.

These data and those presented by Aketa⁴ are evidence that protease-sensitive sperm receptor sites exist on the vitelline layer. Further evidence for this hypothesis comes from the finding that attached sperm can be removed from formaldehyde-fixed eggs by a 10 min incubation in 0.1 mg/ml. pancreatic trypsin.

More conclusive proof that the fertilization protease is involved in degrading the sperm receptivity of the vitelline layer comes from experiments in which unfertilized eggs are incubated in fertilization product. If the protease activity degrades sperm receptor sites, one would predict that incubation of unfertilized eggs in fertilization product would prevent sperm binding and fertilization. If this effect results from the protease action, it should be prevented by adding SBTI to the fertilization product mixture.

To obtain fertilization product, the vitelline layer of *Strongylocentrotus purpuratus* eggs was altered by brief incubation of the eggs in dithiothreitol (DTT)⁵, after which the fertilization membrane does not form and both soluble and

insoluble components of the fertilization product are released into the seawater. (Although it was previously assumed that this treatment removed the vitelline layer, scanning electron microscope observations indicate that the vitelline layer may still be present on the egg surface in an altered form.) Each 25 ml. volume of packed eggs was suspended in 50 ml. seawater and added to 50 ml. of 0.2 DTT in seawater, pH 9.1. The suspension was stirred slowly for 2.5 min and added to 2 litres seawater containing 0.02 M Tris, pH 8.0. The eggs were washed again by resuspension in 1 litre seawater buffered with Tris.

For insemination, each 25 ml. packed volume of the DTT-treated eggs was suspended in 75 ml. of seawater buffered with 0.02 M Tris, pH 8.0, and stirred while 0.1 ml. undiluted semen was added. Buffering the seawater eliminates the large pH change resulting from the release of fertilization acid⁶ and allows for 100% fertilization in such concentrated egg suspensions. Three minutes after insemination the eggs were sedimented gently in the hand centrifuge. The supernatant was removed and centrifuged at 20,000g for 15 min at 0° C to remove sperm and insoluble material. The supernatant fertilization product, containing 1.0–1.2 mg protein per ml., was stored at 0° C until use.

This fertilization product mixture contains a heat-labile activity which prevents sperm attachment to unfertilized eggs and also prevents fertilization. If eggs are exposed to a concentration of fertilization product protein equivalent to that present *in situ* in the perivitelline space (2 mg/ml.), 50% of them become unfertilizable in 5–10 s. After a 30 s exposure to 1.4 mg protein per ml. up to 87% of the eggs will not fertilize. In our standard assay for the inhibitory action of the fertilization product, a protein concentration of 0.5 mg/ml. usually rendered 90–100% of the eggs unfertilizable after a 5 min exposure (Table 2). The action of the fertilization product to inhibit sperm attachment and prevent fertilization can be attributed to the fertilization protease activity since it is prevented by the prior addition of SBTI to the fertilization product (Table 2). Furthermore, the fertilization inhibitor action and the protease activity have similar heat inactivation curves (extent of heat inactivation determined following 2.5 min exposure to various temperatures between 20° and 70° C and assaying for fertilization inhibitor and protease activities at 20° C).

Unpublished results show that the protease activity of the fertilization product does not affect fertilization by any effect on the sperm, but through its effect on the vitelline layer of the unfertilized egg. First, there is no inhibition of fertilization if sperm are incubated in the fertilization product for 30 min and eggs then added to this mixture. Second, scanning electron microscope investigations of eggs incubated in the fertilization product reveal drastic effects on the ultrastructure of the vitelline layer. This effect is not seen if SBTI is first added to the fertilization product. Third, the fertilization inhibitor action is not seen if the vitelline layer is altered by pretreatment of the eggs in DTT or trypsin.

The trypsin-like protease activity released from sea urchin eggs at fertilization appears to have at least two functions,

both involving alterations of the vitelline layer. These are the elevation of the vitelline layer away from the egg surface¹ and the detachment of supernumerary sperm from protease-sensitive sperm attachment sites. Polyspermy is increased if eggs are fertilized in the presence of inhibitors of the protease, such as SBTI. This protease activity may thus prevent polyspermy by destroying the attachment sites of the non-fertilizing sperm; other drugs which increase the incidence of polyspermy, such as nicotine, also prevent detachment of supernumerary sperm. A persistent attachment of sperm before other phases of the block to polyspermy are completed, such as the hardening of the fertilization membrane⁷ and hyaline layer formation⁸, may allow time for multiple sperm entry.

A proteolytic mechanism for the establishment of the block against polyspermy may exist in sea urchins other than *S. purpuratus* and in animals as distantly related to echinoderms as mammals. Treatment of eggs of *Lytechinus pictus* with the fertilization product from *S. purpuratus* prevents fertilization in this species. We have also found protease activity in *L. pictus* fertilization product which exhibits a weak fertilization inhibitor action on *S. purpuratus* eggs. It is of great interest that the fertilization product from the cortical granules of golden hamster eggs contains fertilization inhibitor activity. In this case the hamster fertilization product apparently alters the zona pellucida of the ovum, so that sperm cannot attach to the zona⁹. This coincidence between echinoderms and mammals suggests that these two widely separated animal groups may use similar mechanisms in achieving successful fertilization.

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Note added in proof. Gwatkin *et al.* (*J. Reprod. Fertil.*, in the press) have also found an SBTI-inhibited activity in the cortical granule material from hamster vitelli which induces the zona reaction in unfertilized hamster eggs and renders them incapable of binding sperm.

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Table 2 Fertilization Inhibitor Action of Fertilization Product

Treatment	No. fertilized	No. unfertilized	% Fertilized
FP+eggs	6	106	5
FP+SBTI+eggs	75	7	91
Control eggs	94	2	98

One tenth ml. standard preparation FP was mixed with 0.1 ml. egg suspension (approximately 100 eggs). After 5 min (16° C) 0.01 ml. sperm suspension, prepared by suspending 0.025 ml. undiluted semen in 10 ml. seawater, was added to the eggs. The number of fertilized and unfertilized eggs was counted. Only 5% of the fertilization product treated eggs fertilized compared to 98% of the control eggs. Mixing 0.01 ml. seawater solution of soybean trypsin inhibitor (1 mg/ml.) with 0.1 ml. fertilization product before addition of the egg suspension abolished the fertilization inhibitor action.

Sexual Differences in Human Crowd Motion

In an earlier paper¹ data were presented on the speed/velocity probability density distributions measured for three human crowds. Ideally all the people in any crowd would have about the same mass and the same probability of having given velocity components and we considered that the three crowds studied—university students, pedestrians on a zebra crossing, and 6–8 year old children in a school playground—were homogeneous with males and females present in about equal numbers. Observations were carried out in 1965–1966 on more than 2,000 people (Table 1). The measured distributions agreed

Table 1 Numbers for 1965–1966 and 1970 Observations

	Students	Pedestrians	Children
1965-1966			
Still mode	—	—	185
Walk mode	693	628	377
Run mode	—	9	217
1970			
Walk mode, males	500	623	—
Walk mode, females	636	452	—

Table 2 Summary of χ^2 -squared Tests of the Maxwell–Boltzmann Type Hypothesis

	Students in walk mode		Pedestrians in walk mode	
	Males	Females	Males	Females
χ^2 (d. of f.)	15.1 (9)	12.8 (7)	17.5 (11)	5.40 (9)
% significance	9.0	8.0	10	80

reasonably well with the theoretical distributions obtained from a Maxwell–Boltzmann theory, except near the maximum of each distribution where a significant deviation occurred. The measured distributions all displayed a bimodal character which was tentatively attributed to sexual inhomogeneity among the people. This led to the hypothesis that males and females behaved as though they belonged to different populations in an otherwise homogeneous crowd. The effect was not anticipated during the planning of the observations and the data on the differences between the sexes were not complete enough to test the hypothesis.

We present here some new data designed to make an adequate test of the hypothesis. The observations were made during 1970 and once again more than 2,000 people were studied (Table 1). The data were gathered for the walk mode of the student and pedestrian crowds. From the improved experimental conditions we not only established that the hypothesis was correct but also detected other important differences in the behaviour of the sexes.

The availability of students made it convenient to select them for further study. We also decided to make a further study of the pedestrians in the hope of confirming any new effects, although we were forced to use a different zebra crossing.

Distributions were measured as described in the previous paper, but separate distributions were obtained for males and females (Figs. 1 and 2). For each crowd the male and

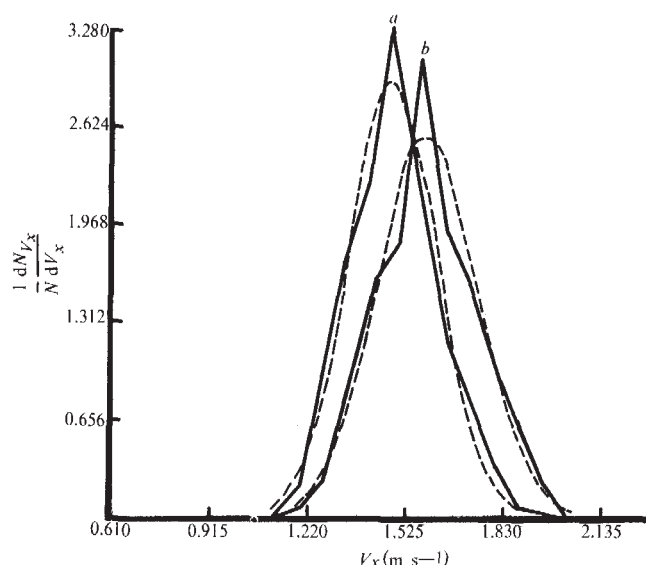


Fig. 1 Probability density functions of the velocity component V_x distributions of 636 female and 500 male students in walk mode on a footpath outside Fisher Library, University of Sydney. *a*, Female data: —, measured distribution; ---, theoretical (Maxwell–Boltzmann) distribution. *b*, Male data: —, measured distribution; ---, theoretical distribution.

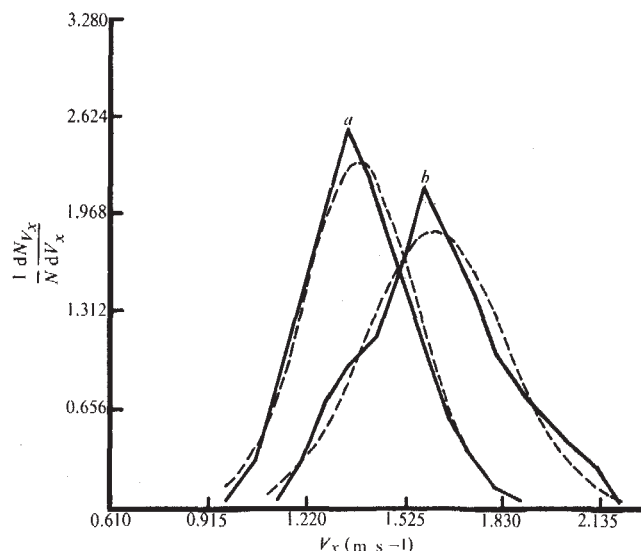


Fig. 2 Probability density functions of the velocity component V_x distributions of 452 female and 623 male pedestrians in walk mode on a zebra crossing at Railway Square, Sydney. Key as Fig. 1.

female curves are displaced from each other by a small but definite amount, and there can be no doubt that the sexes are from different populations. Hence there is sexual inhomogeneity in these crowds. In fact the sexes behave roughly like isotopes* of a crowd species. The theoretical curves obtained from the Maxwell–Boltzmann theory are shown for comparison; their formulas have been given previously¹. The χ^2 test was used to compare the theoretical and observational data (Table 2). For the students the theory cannot be rejected at the 5% level of significance; it is not far off the 10% level and is actually better than this for the pedestrians. The theory should therefore be accurate enough to be useful.

The distributions were tested for skewness and kurtosis by means of the statistics

$$\hat{\gamma}_1 = \Sigma(V_x - \bar{V}_x)^3 / Ns^3$$

$$\hat{\gamma}_2 = (\Sigma(V_x - \bar{V}_x)^4 / Ns^4) - 3$$

where V_x is the velocity component parallel to the footpath in the case of the students, and parallel to the zebra crossing in the case of the pedestrians, $\bar{V}_x$ the mean velocity of translation of the crowd, N is the sample size and s the standard deviation. The null hypothesis is that $\sqrt{N/6} \cdot \hat{\gamma}_1$ and $\sqrt{N/24} \cdot \hat{\gamma}_2$ are unit normal variates. Thus for example if $(\sqrt{N/6} \cdot \hat{\gamma}_1 > 2)$ one would reject the hypothesis at the 5% level. The results are shown in Table 3. The students have no significant skewness at the 5% or even the 10% level, and the hypothesis is accepted for its case. If it is assumed that the first series of observations also has this property then one can identify the twin peaks in the distributions of the first series with the male and female means (Table 4). The means have not changed by any significant amount during the time lapse between the first series 1965–1966 and the second series 1970. The standard deviation of $v_{r.m.s.}$ for the combined student population, however, has changed quite markedly, from 0.20 ms^{-1} in 1965–1966 to 0.16 ms^{-1} in 1970. Now the kinetic energy of the random motion is proportional to $v_{r.m.s.}^2$ and therefore the energy of the student population in walk mode has decreased. The student population has therefore changed its macrostate over 4 to 5 yr and we are not dealing with a strictly stationary process.

By contrast with the students, the pedestrians do have a significant skewness at the 5% level. The hypothesis we favour is that it is due to the challenge of motor vehicles. The effect is to cause most people to slow down when challenged, and a

* This analogy with molecular systems is convenient but somewhat misleading because males tend to have a larger average kinetic energy than females.

Table 3 Skewness and Kurtosis Test of 1970 Observations

	Students in walk mode		Pedestrians in walk mode	
Unit normal variate	$\sqrt{N/6} \cdot \hat{\gamma}_1$	$\sqrt{N/24} \cdot \hat{\gamma}_2$	$\sqrt{N/6} \cdot \hat{\gamma}_1$	$\sqrt{N/24} \cdot \hat{\gamma}_2$
Males (unit s.d.)	0.928	-1.80	2.11	-1.89
% significance	35.5	7.2	3.5	5.9
Females (unit s.d.)	1.60	-1.27	4.49	3.62
% significance	11.0	20.4	<0.1	<0.1

Table 4 Comparison of Means, $\bar{V}_x$, and Standard Deviations, $V_{r.m.s.}$, of Both Series of Observations

		Students in walk mode		Pedestrians in walk mode	
		$\bar{V}_x$	$V_{r.m.s.}$	$\bar{V}_x$	$V_{r.m.s.}$
1965-1966	Males	1.57	—	—	—
	Females	1.42	—	—	—
	Combined	1.53	0.20	1.44	0.23
1970	Males	1.59	0.16	1.62	0.22
	Females	1.49	0.14	1.38	0.17
	Combined	1.54	0.16	1.52	0.23

few to speed up. If this is so then the skewness should depend on the motor traffic density. One striking feature of the skewness is that it is much more pronounced for the females than for the males. Assuming that traffic is responsible, then this means that a motorist finds it much easier to perturb the motion of a female pedestrian on a zebra crossing than that of a male. The skewness prevented us from estimating the male and female's means for the first series of observations. It will be noted from Table 4, however, that the mean of the combined populations is slightly greater for the second series, while the standard deviation is about the same. If we can disregard the fact that the two series of data were obtained from different zebra crossings then we can say that the random processes appear to be stationary.

The kurtosis of the pedestrian distributions provides another example of the differences between the sexes. For male pedestrians (and students) it is negative and not quite significant at the 5% level. For the female pedestrians the kurtosis is positive and significant to a high level, and may indicate a lack of homogeneity in the female population, which may be a mixture of perhaps several female species with different means. The female pedestrians were observed to have a broader age group than the males. On the other hand, the negative kurtosis of the males, especially since it is found in both populations, may indicate a minor loss of statistical independence caused perhaps by a certain degree of common purpose in their activities. The effect, however, is fairly small, and more refined observations are needed to test these hypotheses.

It has been found that males and females cause a definite inhomogeneity in an otherwise homogeneous crowd, and this makes it necessary to count the sexes as though they are in different populations or "isotopes" of the same crowd species. If the homogeneity of the crowd is established in terms of sex, age, and uniformity of activity and environment of the people, there is good agreement with a theory of the Maxwell-Boltzmann type, even though the students showed signs of being not quite in equilibrium, and the pedestrians a certain skewness and kurtosis in their distributions. Hence, there seems to be reasonable grounds to conclude that: (i) The males and females of an approximately homogeneous crowd "gas", either in equilibrium, or nearly so, and composed of statistically independent prime particles, will separately obey Maxwell-Boltzmann type statistics in two-dimensional velocity space, for the fluctuating part of their motion. (ii) For female pedestrians the distribution is significantly skewed compared to male pedestrians. This apparently indicates that female pedestrians are much more easily perturbed in their motion than males.

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Wool of the 14th Century BC from Tell el-Amarna, Egypt

THE type of sheep kept in ancient Egypt has been deduced mainly from pictures. In the Old Kingdom (2700-2200 BC) black, white and piebald animals are depicted with corkscrew horns¹; the coat was apparently short suggesting an outer coat of kemp, and an under coat of wool as in modern African so-called "hair" sheep, the coat of which is little different from the double coat of wild sheep. The picture chronology indicates that in the Middle Kingdom (2134-1647 BC) this sheep was replaced by a woolly one with the usual horn shape². I have described³ a sample of raw wool from the Old Kingdom which, although coarse, did not have the discontinuous diameter distribution expected in a double-coated sheep (Table 1). I have examined samples from the agricultural museum in Cairo, comprising some raw wool and a length of yarn. They were from the temple stores of Akhenaten at Tell el-Amarna; this pharaoh lived during the 14th century BC, that is, in the 18th Dynasty (1570-1320 BC) which was in the New Kingdom.

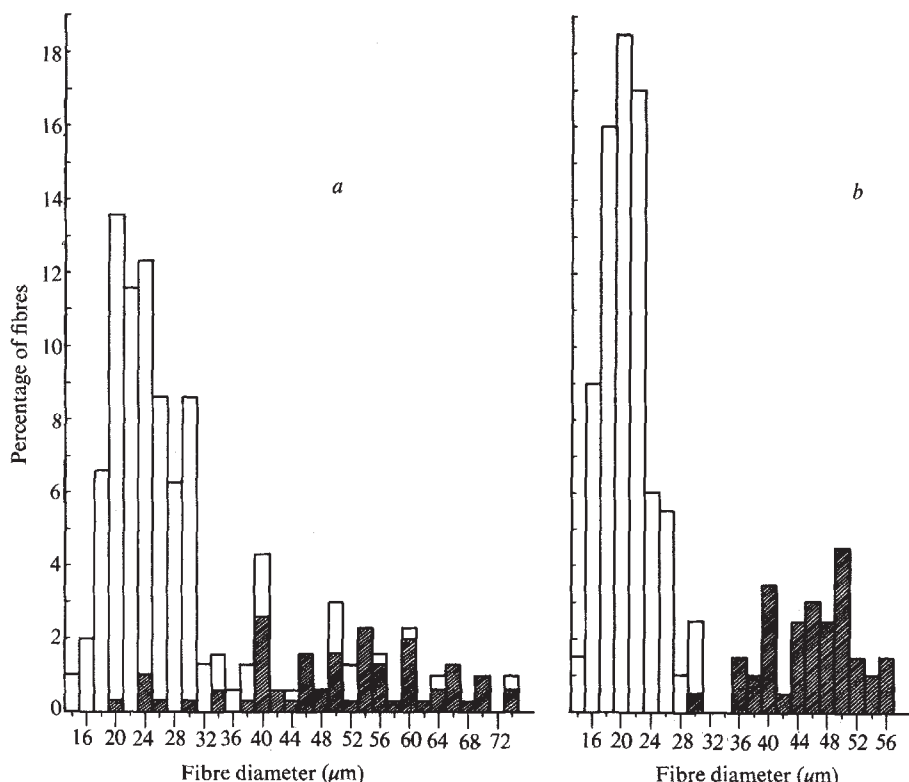
The wool had pigment in only one or two of the coarser fibres. The yarn varies from 0.5 to 1.0 mm in diameter and was tightly spun in an anti-clockwise (S) direction, which is usual for this region. The raw wool comprised two samples of uncertain origin, one being somewhat longer, with a straighter staple tip, than the other. Microscopic examination revealed fine and medium wool fibres, and relatively fine hairs with a non-latticed medulla, but no kemps. Two fibre diameter determinations were made on each sample, and one on the yarn. The International Standards Organization method 181 was used to measure 100 fibres in each determination¹. The repeat measurements gave similar results, and the yarn was similar to the straight-tipped staple, which contained coarser fibres than the curly-tipped staple (Table 1).

Each sample had a primitive skewed-to-fine diameter distribution, the coarser one being comparable with what has been termed the "hairy medium wool", such as the hairy Soay, and the other being comparable with the generalized medium wool, as in the woolly Soay and "fine" wool found in the Middle East 2,000 years ago³. The similarity of the means and modes indicates that the difference between the samples was not great, being brought about by the presence or absence of hairs. On the other hand the histograms (Fig. 1) indicate different diameter distributions, and a spread of medullated fibres into the fine group of the straight-tipped sample. The same evolutionary loss of hair would be indicated even if the two samples had come from the same fleece; part of the fleece would have lost the hair. The two kinds would fit the grades "good quality" and "second quality" mentioned in Babylonian records⁶.

This Egyptian wool was most like the Pazirik and Hallstatt samples of Iron Age wool³, but whereas these were hairy, the lack of hairs in the Egyptian curly-tipped sample pushes back the existence of the generalized medium type 1,400 yr.

None of these samples had the discontinuous diameter distribution of the wild sheep with under wool having a mode of 14 μ m and an outer coat with a mode of 160 μ m³. The Pazirik sample had a discontinuous diameter distribution but no kemps, and the Hallstatt sample had kemps but with a continuous distribution. The present samples provide further

Fig. 1 Fibre diameter distributions of the two raw wool samples investigated. *a*, Straight-tipped; *b*, curly-tipped. Shaded parts indicate medullated fibre.



support for the constancy of diameter in fine fibres noted previously^{4,5}. The mode in Bronze Age wool³ ranged from 14 to 18 μm but since that time the mode of fine fibres has been about 20 μm .

The Pazirik and Hallstatt samples in fact comprised skin with the wool attached; within the skin the fibres had brush ends indicating a natural moult and the likely collection of the wool by plucking. Only one brush end was observed in the present samples suggesting that the wool had not been plucked; it would be extremely difficult to pluck a sheep unless brush ends had formed, but there were an appreciable number of the roots of pulled fibres, the remainder appearing like worn cut ends. Shears are thought to have been invented about 1000 BC⁶, so it may be that this Egyptian wool had been cut off with a knife, the root ends belonging to fibres that had been dragged out by the knife instead of being cut. A knife was

used in this way in north-west Britain recently during the decline of plucking and before the introduction of shears.

I thank Mr I. L. Mason for drawing my attention to this wool, officials of the Agricultural Museum, and the Ministry of Agriculture, in Cairo, for supplying the material, and Mr Cyril Aldred for reading the manuscript and correcting the dates.

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Table 1 Wool Measurements

	Staple length	Fibre diameter (μm)		Medul- lated fibres	% Pig- mented fibres
		Range	Mean Mode		
Yarn	—	18–66 74*	33.2 22	16	0
Straight (a) tip	7 cm	18–70	33.9 20 and 24	33	2
(b)	7 cm	14–46 50, 70, 74*	26.1 20	15	0
Curly (a)	6 cm	14–54	26.0 22	21	0
tip (b)	6 cm	14–56	27.0 20	26	0
Overall range		14–74			
		Means	29.2 21.2	22	0.4
Other samples for comparison ³					
Old Kingdom (ca. 2500 BC)	(2)	12–110	34.6 20	21	1
Pazirik (400 BC)	12	12–42 62–82*	28.2 22 and 70	14	0
Hallstatt (500 BC)	(3)	20–80 96, 100*	35.5 22	22	0

* Diameters of individual fibres occurring outside the main range are listed separately.

Effects of Polychlorinated Biphenyl on Marine Phytoplankton Communities

WE report here the first *in situ* bioassay of short term effects of polychlorinated biphenyl compounds (PCBs) on natural marine phytoplankton communities. (Synthesis and utilization of these industrial organochlorines have been discussed before^{1–5}.) PCB residues have been identified in water, biota and sediment samples throughout the marine environment^{5–9}. Near Escambia Bay, Florida, industrial outfall water and sediment in the immediate outfall vicinity contained PCB residues as high as 275 p.p.b. (1 billion = 10^9) and 486 p.p.m. respectively. Live oysters approximately 10 miles downriver of the outfall contained residues as high as 3 p.p.m.¹⁰. Seven of ten municipal wastewater outfalls sampled along the California coastline were confirmed to carry PCB compounds¹¹.

Information about the physiological and ecological effects of PCBs in natural aquatic communities is limited.

We have investigated the effect *in situ* of a range in PCB concentrations to photosynthetic radiocarbon uptake in natural communities of phytoplankton. Experiments were conducted in St George Sound in the northeastern Gulf of Mexico near Franklin County, Florida, during August–November 1970 and February–April 1971. Phytoplankton communities of St George Sound were quite heterogeneous, showing considerable variability even at a single station with less than 1 h between samplings. East–west tidal currents through the sound continually renewed water sampled and were felt to be the principal source of this sampling variability. Among the phytoplankton, diatoms were the most predominant group and were most frequently represented by the following genera¹²: *Cerataulina*, *Chaetoceros*, *Biddulphia*, *Eupodiscus*, *Leptocylindrica*, *Melosira*, *Nitzschia*, *Rhizosolenia*, and *Thalassionema*. Dinoflagellates were generally present as a few individuals of *Peridinium* and *Ceratium* were observed in most samples. Coccolithophores and silicoflagellates have been reported in the northeastern Gulf of Mexico samples but were not noted in this study¹². The PCB compounds studied included 2,4'-dichlorobiphenyl, 'Aroclor 1242' and 'Aroclor 1254'. The two types of 'Aroclor' are isomeric mixtures of pure chlorinated biphenyl and differ by their respective degrees of chlorination: 42% and 54% 2,4'-dichlorobiphenyl is a pure PCB compound with 31.8% chlorine by weight and constitutes part of 'Aroclor 1232'. PCB effects were compared with *p,p'*-DDT, the major chemical constituent of commercially applied DDT formulations.

For each experiment a single 10-l. water sample was taken at 0.5 m to avoid sampling the surface film, shaken to homogenize plankton distribution and siphoned into duplicate or triplicate 240 ml. bottles. After the addition of seawater to the sample bottles, concentrations of organochlorine in ethanol solvent were added using 10 microlitre capacity syringes. The indicated organochlorine concentrations represent the initial concentrations and do not reflect any partitioning of organochlorine between plankton, suspended solids, bottle walls and water. Concentrations of ethanol less than 100 p.p.m. were not found to inhibit radiocarbon uptake; the maximum concentration of ethanol employed was 50 p.p.m.

In each experiment twenty-four to thirty bottles were filled and fastened in triads across a rectangular frame. The assembled frame was suspended from the surface by supporting buoys at approximately 1.5 m. After the 24 h organochlorine incubation period 5 μ Ci of $\text{NaH}^{14}\text{CO}_3$ was added to the bottles. The frames were reassembled and resuspended *in situ* for 4 h. To terminate the experiments, 1 ml. of buffered formalin was added and the samples were filtered through 47 (0.45 μ m) membrane filters. After drying the filters over silica gel pads for 4–7 days, the radioactivity was determined by proportional counting technique. The results are reported as percentage uptake of radiocarbon by phytoplankton exposed to various concentrations of PCB relative to control populations.

The effects of *p,p'*-DDT and 2,4'-dichlorobiphenyl are compared in Fig. 1a. The toxicity of these compounds to phytoplankton is strikingly similar. DDT and 2,4'-dichlorobiphenyl inhibited phytoplankton radiocarbon uptake at 5 p.p.b. and 7 p.p.b. respectively. LC_{50} values for *p,p'*-DDT and 2,4'-dichlorobiphenyl were 35 p.p.b. and 30 p.p.b. respectively¹⁰. Wilcoxon two sample rank tests did not show differences between the toxicity of these compounds. 'Aroclor 1242' and 'Aroclor 1254' results are compared in Fig. 1b. Both isometric blends of polychlorinated biphenyl displayed greater toxicity to phytoplankton radiocarbon uptake than did either of the pure compounds, *p,p'*-DDT or 2,4'-dichlorobiphenyl. 'Aroclor 1242' and 'Aroclor 1254' were both toxic to phytoplankton populations at levels as low as 1–2 p.p.b. and showed LC_{50} values of 6.5 p.p.b. and 15 p.p.b., respectively. These concentrations are considerably below published 0.3–1.5 p.p.b. seawater solubility of 'Aroclor 1254'¹³.

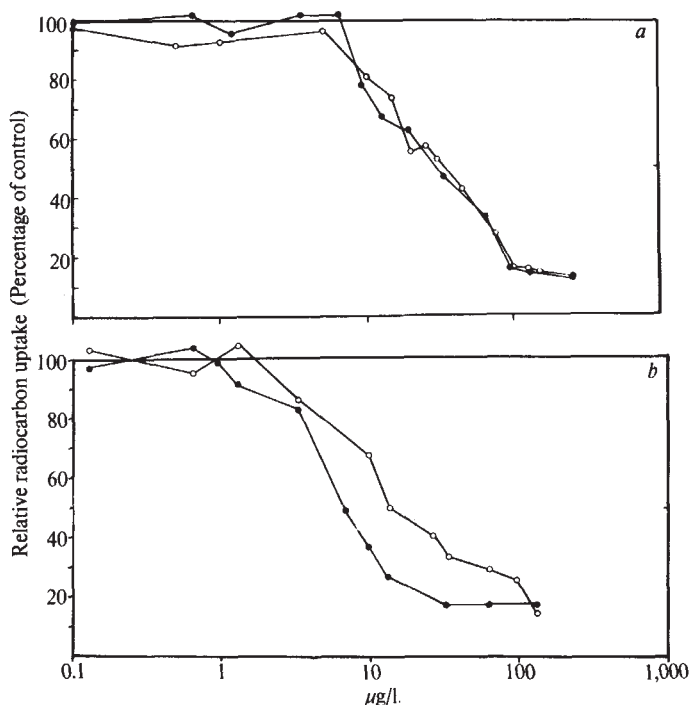


Fig. 1 Effect of organochlorines on *in situ* radiocarbon uptake by marine phytoplankton communities. a, *p,p'*-DDT (○) and 2,4'-dichlorobiphenyl (●); b, 'Aroclor 1242' (●) and 'Aroclor 1254' (○). See text for explanation.

'Aroclor 1254' is more toxic than an equivalent amount of DDT to the growth rates of two marine centric diatom species, *Skeletonema costatum* and *Thalassiosira pseudonana*, in laboratory cultures¹⁴. Keil and others¹⁵ demonstrated that the marine diatom *Cylindrotheca closterium* sorbed and concentrated 0.1 p.p.m. 'Aroclor 1242' up to 1,000 times with subsequent inhibited growth and diminished levels of chlorophyll and nucleic acids in laboratory cultures.

Inhibition of photosynthesis and growth of marine phytoplankton single-species cultures in the presence of organochlorine pesticides has been described before^{16,17}. In natural phytoplankton communities all stages of growth are represented through various phytoplankton species. One might expect a natural community to exhibit PCB effects at lower concentrations than found in vigorously growing, pure cultures. While laboratory cultures are grown in controlled conditions ideally suited for that optimal growth rate, natural populations are subjected to various community stresses which may render plankton species more susceptible to organochlorine stress. Succession by one of two phytoplankton species in the presence of 'Aroclor' concentrations has been shown in laboratory conditions where levels as low as 1 p.p.b. were reported to alter competitive interactions between *T. pseudonana* and *Dunaliella tertiolecta* in culture¹⁸. The effects of PCBs reported in our studies suggest that the toxic effects of organochlorine are more acute *in situ* at a community level than for single species laboratory cultures. Any effort to establish water quality standards for PCB compounds should consider possible chronic effects on natural communities.

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Organochlorine Levels in Atlantic Seabirds

SYNTHETIC organochlorine derivatives have been found in a variety of seabirds from the eastern North Pacific¹ and even the Antarctic², and considerable amounts of polychlorinated biphenyls (PCBs) in particular have been found in birds from the Baltic³, Dutch waters⁴ and the livers of starving common guillemots, *Uria aale*, washed ashore around the Irish Sea in the autumn of 1969⁵. But comparatively few estimates of the amounts present in normal seabirds from the open North Atlantic have yet been published. We are reporting elsewhere⁶ the results of the investigation of over 100 birds from a representative range of species occurring between Scotland and the Arctic. In general they were all found to contain mainly PCBs and *pp'*-DDE, the former in greater concentration in every bird examined, in marked contrast to the North Pacific, where DDE levels are normally higher¹. The ratio of PCB to DDE was usually between two and ten in all species with the exception of the kittiwake, *Rissa tridactyla*, where the ratio was always higher. It is difficult to measure DDE in the presence of relatively high amounts of PCBs but in some of the kittiwakes this ratio was at least 60. This suggests that kittiwakes have a food source relatively high in PCBs compared with the other species or, less likely, that they metabolize or excrete DDE more rapidly than the other species.

The total organochlorine content in muscle and liver was generally low in the auks and the few shearwaters examined, between 0.1–1.0 parts per million. The levels were higher in more pelagic species, in the kittiwake, fulmar, *Fulmarus glacialis*, and British storm-petrel, *Hydrobates pelagicus*, generally 1–10 p.p.m. and the highest levels rising above 10 p.p.m. were found in large gulls and skuas feeding largely around trawlers in the winter and seabird breeding stations in the summer. In all the birds caught or shot the liver levels were normally of the same order as those in muscle, whilst in the birds found dead on beaches the liver levels were normally markedly raised compared to those in muscle. A common guillemot chick only contained a tenth the amount found in the adult accompanying it, so that it would appear that the organochlorine content is not inherited. The stomach oil of a fulmar which it feeds to its chick contained only 1.3 p.p.m. PCBs compared to 1.5 p.p.m. in the liver, 2.3 p.p.m. in the muscle and 42 p.p.m. in the fat, so that it appears that the parent does not pass on the levels found in the fat.

There was no substantial difference in the organochlorine content of birds obtained around the north of Scotland, the Faroes, and in the Barents Sea, and only the puffins *Fratercula*

arctica began to show a diminution in the Davis Strait. Indeed, the highest levels were found in glaucous gulls, *Larus hyperboreus*, breeding above the seabird colonies on the cliffs of Bear Island. Normal birds, which appeared from regurgitated pellets to have been feeding largely on other seabirds' eggs, had an average content of 24 p.p.m. PCBs and 17 p.p.m. DDE in the liver, and one had 535 p.p.m. PCB and 67 p.p.m. DDE in the fat. Another bird showed a curious weakness and failure of coordination of the limbs, especially the legs, so that it fell sideways and threw itself about in a convulsion when it was approached so that it was easily caught by hand. On dissection, it was wasted and underweight but otherwise appeared normal. Its liver contained 311 p.p.m. PCBs and 67 p.p.m. DDE. Experimentally poisoned finches showed similar neurological signs and were found to have a mean liver PCB content of 345 p.p.m. at a dose rate sufficient to cause 50% mortality⁷. Thus this level of 311 p.p.m. PCBs in combination with DDE, which is also known to cause increased retention of PCBs mobilized from fat in starving robins *Erithacus rubecula*⁸, is a possible cause of the symptoms. Whatever the cause of the bird's decline in condition and convulsions, it seems a matter for concern that such high levels of organochlorine can now be found in the Arctic over 250 miles north of the mainland and in a species which never migrates far south in an area where the surface seawater probably does not even originate in Europe, but in the western North Atlantic.

Specimens used in this study were collected during an investigation of the ecology of birds at sea, financed by the Natural Environment Research Council. We thank Professor E. Brun of Tromsø University who arranged for the second author to visit Bear Island, and Mrs C. Gallacher for technical assistance.

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PCB Contamination from Boat Bottom Paint and Levels of PCB in Plankton outside a Polluted Area

SAMPLES of zoo- and phytoplankton were collected in Stockholm harbour during an investigation into levels of polychlorinated biphenyls (PCBs). The samples were taken in September and October 1971 and in June 1972 at the localities shown in Fig. 2a and b. The opportunity was taken to check the possibility that contamination of plankton samples might occur from the anti-fouling paint, containing PCB, used on the boat.

Plankton samples were taken at various locations around the boat in a 0.1 mm mesh net at a trawling speed of about 1 knot. They were analysed within three days of collection. PCB contents were determined in terms of the extractable fat because of the difficulty in determining the true sample weight.

The plankton was homogenized and extracted as described previously¹; extracted fat was weighed and dissolved in

hexane. 1 g of stirred paint was transferred to a test tube and extracted twice with 10 ml. portions of hexane. These solutions were shaken with concentrated sulphuric acid, containing 7% sulphur trioxide and, after dilution with hexane, injected into a gas chromatograph, equipped with an electron-capture detector¹. Not all PCB components present in the plankton are reported, because of interference in PCB determination by substances such as DDT and its metabolites. The omitted PCBs represent about 30% when referred to a standard ('Clophen A50') (Fig. 1).

The boat paint and plankton samples from June 1972 were also analysed for the presence of chlorinated phenols. The extract (5 ml.) was shaken with 1 M potassium hydroxide (5 ml.) and the hexane phase discarded. After washing the water phase with hexane, phosphoric acid was added (to pH < 3). Hexane (5 ml.) was added and, after shaking, the organic phase was treated with diazomethane, and the extract injected into the gas chromatograph. The boat paint extract was also analysed for polychlorinated dibenzo-p-dioxins and its precursors, as described previously².

Plankton was collected in October 1971 and June 1972 simultaneously amidships (7 foot from the boat) and in the wake, and in June 1972 also in front of the boat from the bowsprit. In October 1971 the plankton samples in the wake contained about 4 times more PCB than the samples from beside the boat. In June 1972 there was a 14-fold difference (see Table 1). This increase can be explained by the lower density of plankton in June; less fat is exposed to the same amount of PCB in the passing water giving a higher concentration in the fat. When plankton with lipophilic membranes fill up the meshes of the net, this will act as an active bed, through which the PCB-contaminated water from the bottom of the boat has to pass.

Gas chromatographs of PCB from the wake of the boat were very similar to those of the bottom paint; this was not

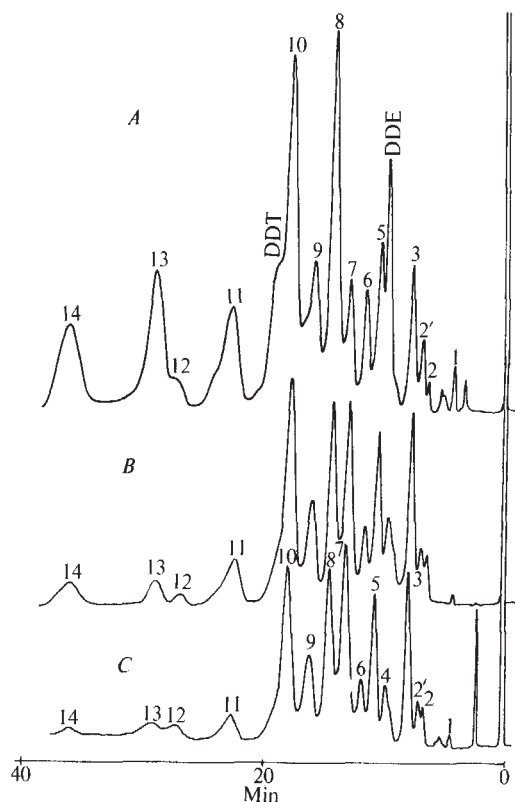


Fig. 1 Three gas chromatograms showing the differences in distribution pattern of PCB components (peak 1-14) in: A, plankton from the bowsprit extract undiluted; B, from the wake extract undiluted; C, boat paint, 'Clophen A50'. Column: mixture (3:1) of SF 96 (4%) and QF 1 (8%) on 'Chromosorb W'. Column size: 160 cm all glass with an inner diameter of 0.18 cm.

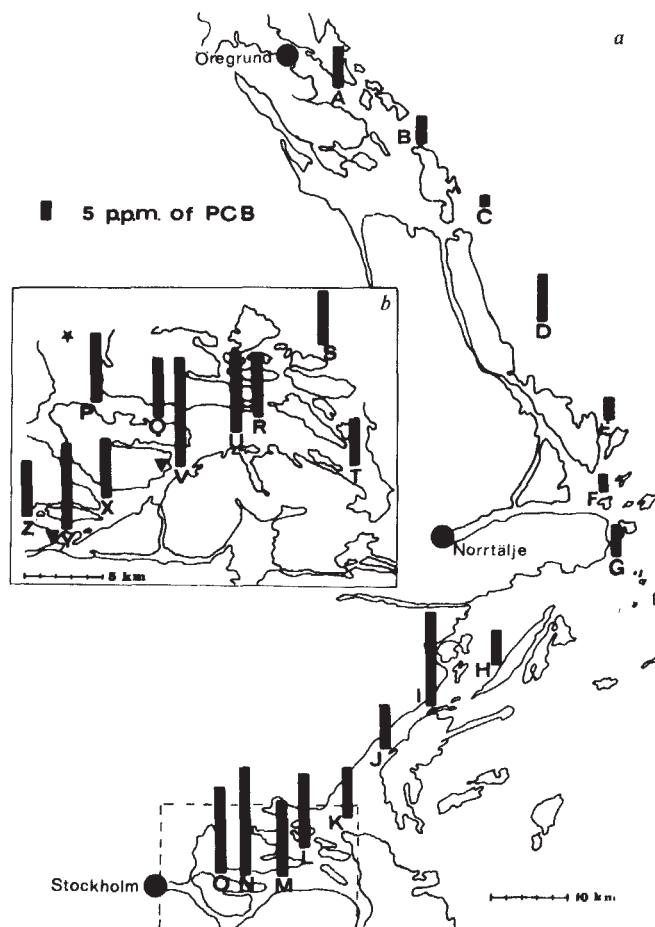


Fig. 2 Levels of PCB in extractable fat of plankton from the archipelago of Stockholm. Capitals stand for sampling localities. The bottom of the bar represents the sampling locality. a, Sampling localities in September 1971; b, sampling localities in October 1971 (the inner part of the archipelago, see a). ★, Samples taken in June 1972; ▼, sewage treatment plant.

true with the PCB in the samples from amidships and ahead (Table 2). This is consistent with contamination from the boat affecting samples taken astern. Furthermore, Fig. 2 shows that DDT and DDE detected in the bow sample cannot be seen in the stern sample because of PCB contamination—the relation between DDT and PCB compounds has been changed.

The difference between the levels in side and bow samples may be due to weighing errors, caused by the small amount of plankton in the June samples (only about 10 mg of fat was extracted). The compositions of the PCBs in these two

Table 1 PCB in Plankton Sampled Simultaneously at Different Points in Relation to the Boat (p.p.m. on Fat Weight Basis)

Locality date	Astern	7 foot amidships	From the bowsprit
S. October 1971	80	18	No collection
★ June 1972	170	12	7.6

Table 2 PCB Components in Plankton taken at Different Places around the Boat and in Paint in June 1972

Sampling places from the boat	Different PCBs (2-13) in percentage of the total PCB amount									
Plankton										
In front of the boat,										
from the bowsprit	12	12	8.4	5.5	23	14	6.4	3.9	16	
7 foot abeam of the boat	19	14	9.0	6.1	20	11	5.6	3.4	13	
From the wake of the boat	18	23	7.9	12	17	12	4.2	1.9	3.9	
Paint	21	25	6.7	15	15	9.9	2.7	3.3	2.2	

Table 3 PCB in Plankton Samples from Different Localities (p.p.m. on Fat Weight Basis)

Date	Locality	Total amount of PCB compounds	Different PCBs (2-13) in percentage of the total PCB amount								
			2	3	6	7	8	10	11	12	13
Sept 1971	A	13	11	18	8.3	9.8	21	12	3.8	4.3	13
Sept 1971	B	8.2	12	18	8.5	11	21	12	3.5	4.2	11
Sept 1971	C	3.1	11	18	9.2	9.3	20	12	4.0	4.7	12
Sept 1971	D	15	5.7	13	11	7.3	23	13	5.1	4.6	18
Sept 1971	E	6.7	9.2	13	9.7	9.0	24	12	4.3	3.1	16
Sept 1971	F	5.2	6.4	11	9.1	8.5	23	12	4.9	4.7	20
Sept 1971	G	9.5	9.4	17	9.4	12	23	12	3.2	4.2	10
Sept 1971	H	11	7.6	15	10	7.3	23	13	4.2	4.1	15
Sept 1971	I	30	7.8	16	5.7	7.1	29	14	3.1	3.4	14
Sept 1971	J	14	14	18	7.3	6.9	24	12	2.6	3.4	12
Sept 1971	K	16	11	15	8.8	5.5	24	12	4.6	2.8	16
Sept 1971	L	24	20	17	8.2	6.0	21	10	3.2	1.7	13
Sept 1971	M	24	22	19	8.8	7.0	20	9.7	2.5	2.2	9.1
Sept 1971	N	35	16	17	6.4	7.1	25	12	2.8	2.8	11
Sept 1971	O	28	26	22	8.6	7.8	18	8.8	2.0	1.3	5.6
Oct 1971	P	22	21	19	8.9	6.4	20	10	2.6	1.4	11
Oct 1971	Q	19	24	23	10	6.3	18	8.9	2.7	—	7.2
Oct 1971	R	20	19	19	10	7.1	20	9.8	2.8	2.4	9.7
Oct 1971	S	18	12	23	9.4	7.6	19	10	—	7.2	12
Oct 1971	T	15	15	24	8.9	6.5	21	14	1.8	2.2	6.0
Oct 1971	U	25	22	20	8.5	10	17	9.9	3.0	3.5	6.5
Oct 1971	V	35	23	19	8.9	9.4	17	9.4	3.3	3.4	7.0
Oct 1971	X	19	28	20	9.0	7.7	17	8.0	2.1	1.8	6.7
Oct 1971	Y	27	31	22	7.9	7.1	16	7.9	1.9	1.1	5.8
Oct 1971	Z	18	19	20	11	7.8	18	9.1	3.6	3.1	9.1

samples were similar (Table 2) and thus for a preliminary study of the PCB contamination in the archipelago³ it seems preferable to use the values of September and October 1971 when the samples were collected beside the boat.

Table 3 and Fig. 2a and b show a decrease of the levels of PCB in plankton in the peripheral parts of the archipelago. This is in agreement with investigations carried out on pike (*Esox lucius*) from the archipelago, which show a similar decrease from the vicinity of Stockholm towards the periphery⁴. In October 1971 the highest levels in plankton are found in the inner part of the archipelago outside two sewage treatment plants in Stockholm (Fig. 2a and b). Upstream of these plants, which is mainly water from Lake Mälaren, the level again decreases (locality Z). The concentration of plankton, however, also decreases and thus there is a decrease in the amount of available fat per water unit, which may result in proportionately a higher concentration of PCB in the fat in the periphery. Thus the difference of the PCB contamination is greater than the values indicate. (The species composition of the plankton samples differs between the samples. Here, for a preliminary study of PCB pollution, this has been considered of minor importance.)

Plankton from the polluted Clyde in Scotland showed values from 40–230 p.p.m.⁹ on a wet weight basis⁵. With an average fat percentage of 1% this corresponds to 0.4–23 p.p.m. on a fat weight basis, which is in good agreement with the range from 0.3–35 p.p.m. found by us.

Furthermore there is a difference between PCBs in the inner and the outer part of the archipelago (Table 3). The highest percentage for peak 2,2' + 2'-tetrachlorobiphenyls (Fig. 2) is found in the inner part of the archipelago in both September and October 1971. This may be due to different sources or to a more rapid metabolization of some PCBs by organisms. That PCB components can be metabolized to some extent has been shown in the rat^{6,7}, where the lower chlorinated PCBs are most easily degraded.

Our investigation shows that samples collected in the wake of the boat are strongly contaminated with PCB from the bottom paint, and may explain the high PCB levels reported from some sea plankton⁸. That commercial bottom paints have such an extensive and continuous loss of PCB makes it also probable that the archipelago of Stockholm is polluted, not only from sources on the shore, but also from boats. The serious PCB pollution of the Baltic may to some extent depend

on the traffic of boats and ships, and it also seems possible to use the proportion of PCB compounds as a criterion of different sources of pollution and biological metabolism. With a proper sampling technique, it seems possible to use plankton for the study of PCB pollution.

Besides PCB the bottom paint also contained 2.3% of pentachlorophenol, 0.20% of 2,3,4,6-tetrachlorophenol and chlorinated 2-hydroxy-diphenyl ethers (precursors of chlorinated dibenzo-p-dioxins). The amounts of predioxins corresponding to octa- and heptachloro-dioxins were 49 and 31 p.p.m. It seems probable that these lipophilic substances will behave in the same way as PCB and subsequently will also pass from the paint into the plankton. This was indeed indicated in three plankton samples collected in June 1972 in front of, beside and behind the boat, where the levels were highest in the wake sample. A general conclusion may therefore be that plankton samples taken in the wake of boats will be contaminated with lipophilic substances in boat bottom paints.

We thank Mrs Waltraud Persson at the Swedish Museum of Natural History for carrying out calculations and for drawing the figures. This investigation was supported by grants from the Swedish Environmental Protection Board.

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BOOK REVIEWS

Without a Word

Non-Verbal Communication. Edited by R. A. Hinde. Pp. xiii+443. (Cambridge University: London, April 1972.) £5; \$17.50.

MAN is such a talkative creature. We easily overlook the fact that communication without words is the sole mode of social intercourse in the rest of the animal kingdom and is still extensive in human interactions. Between 1965 and 1970, a Royal Society study group of psychologists, anthropologists, ethologists and others met more than a dozen times to discuss the nature of non-verbal communication in animals and man. *Non-Verbal Communication* was produced by several members of that group. The authors describe some of the diverse phenomena that are referred to as non-verbal communication, discuss some of the difficulties of terminology and classification that arise in its study, and air some points of current controversy.

Certainly there is diversity enough for anyone, here. The volume runs the gamut from engineering control theory to Botticelli painting, with side trips to look at dancing bees, embarrassed Polynesians, smiling children, grinning lemurs, copulating spiders, flashing fireflies, and smelly ants. The fifteen chapters are divided into three sections: "The Nature of Communication," "Communication in Animals," and "Non-Verbal Communication in Man." After each chapter or section, Professor Hinde provides perceptive editorials, based in part on comments by other members of the group.

In the last few years, several other reviews of non-verbal communication have been written and a large, multi-authored survey on the same topic has recently been published (*Animal Communication*, edit. by Thomas A. Sebeok, Indiana University Press). As a result, most of the concepts and descriptions in this volume have been published elsewhere, in several instances by these same authors. The impression given by such repetition is of a mature research area, tidying up loose ends and undergoing consolidation. Nothing could be further from the truth. The

study of communication in man and animals is a fledgling field, dominated by description rather than analysis, and with more promise than accomplishment.

Non-Verbal Communication surveys what has been accomplished so far. The better chapters, such as Thorpe's review of research on bird calls, Cullen's chapter on principles of animal communication, Andrew's on the sources of mammalian displays, and those by Blurton Jones and Argyle on human interactions, provide perceptive surveys of our present knowledge of communication processes in man and animals. The chapters by Miller and Gombrich remind us that painters and actors too are, in their way, students of non-verbal communication. The outstanding merit of the volume is in reflecting the growing rapport between linguistic and behavioural research, not only in Lyon's and Leach's chapters on language and culture, but in many other places as well. The principal shortcoming of the volume as a whole is that it fails to deal in a fundamental and original way with any basic problem in the study of non-verbal communication. What follows is a small sample of such problems that have been treated inadequately in this volume, or not at all.

Methods for sampling and recording communicative interactions in social groups are virtually ignored in this volume. Two kinds of question are involved. Theoretically, what interaction sampling methods can provide the kinds of data that will enable us to answer our questions about communicative interactions? And in practice, which of the theoretically satisfactory methods are feasible under existing research conditions, and how can the requisite data be encoded and recorded? Methods for describing signal parameters are neglected here. There is no mention of techniques, such as the Eshkol-Wachmann notation or other globographic methods, that will enable us to do for visual signals what a sound spectrograph enables us to do for acoustic signals, or biochemical analysis for pheromones. The book gives no

indication of how to draw up an exclusive and exhaustive ethogram at any nontrivial level of classification. It contains no substantial contribution to the problem of establishing basic units of communicative behaviour, though several authors comment on the effects of the choice of units on subsequent analysis. These are all problems that are fundamental to the study of animal communication, that virtually every student of social interactions faces, and for which the existing literature provides little guidance.

Several authors mention *en passant* some of the available methods for quantitative analysis of communicative interactions, but no survey of such methods is presented, much less any contribution to important unresolved problems, such as determining the statistical stationarity of social communication, estimating repertoire size, or utilizing data from sequences of variable length.

The comparison of human and animal communication in chapter 2 is replete with misunderstandings of the so-called design features. Those interested in such structural analysis of communication processes should consult the original sources¹⁻³.

The bee dance is one of the most extensively studied forms of behaviour in the animal kingdom, yet its signal function is now a source of controversy: Wenner and his colleagues believe that foragers recruited to new-found nectar sources locate the source by means of olfactory cues and not by means of the information that is available in the dance. This is one of the few non-sterile controversies in the study of animal behaviour. It turns on the design of experiments and the nature of evidence and deserves close attention by all students of animal behaviour because, however it is resolved, the standards of acceptable evidence for any putative social signal will be improved. Wenner's booklet⁴ will enable the reader to appreciate the nature of the problem, which—contrary to the impression given in *Non-Verbal Communication*—will not be resolved by

citing the consensus of present opinion, by pointing out that the work of von Frisch *et al.* has produced overwhelming quantities of data in some twenty-five years of work, or by citing ambiguous experiments as final substantiation.

A recurrent theme in *Non-Verbal Communication* is the "causation" of displays. The analysis of causation, when presented by ethologists in terms of motivations, tendencies, and conflicting drives, is sterile reification. We say that a bitch is in heat, in oestrus, or sexually receptive if she presents her perineum to a male, and so forth. What does it mean, then, to say that the bitch presented because she was in heat? To do so is to allow labels to become causes. It is too easy to forget that terms like *receptive*, *angry*, *fearful*, *hungry* and the like are dispositional terms: they are statements that certain classes of behaviour are likely to be exhibited by the organism.

Perhaps students of animal behaviour should completely abandon such terminology in the causal analysis of behaviour until and unless they can demonstrate that analysis in terms of underlying physiology, anatomy, neurology, genetics, past experience, or other causes of behaviour, cannot be conducted without it. At present, such verbiage only clutters the literature—and the mind. Such terminology obscures the fact that strange and important behavioural phenomena are often involved. By not providing operational definitions of such processes, without recourse to hypothetical constructs, the authors of this volume missed an opportunity to make an important contribution to the study of displays. For example, consider conflict situations, a topic that comes up repeatedly in this volume and in the ethological literature. It will not do to describe these in terms of conflicting drives. An operational definition might go something like this. A conflict situation occurs when an animal is simultaneously presented with two signals that by themselves elicit responses that are physically incompatible; physical incompatibility usually hinges on the fact that an object cannot be in two places at the same time. An observation that has been made repeatedly by ethologists is that, under these circumstances, animals often do neither of the incompatible responses but some quite different kind of behaviour, which is then said to be "out of context" or "displaced". These displaced activities often have a special communicative significance.

A vigorous hybridization has recently begun between behavioural biology and contemporary research on population ecology and evolutionary strategies. Instead of the facile phylogenies and *scala naturae* that are presented in this volume, we need systematic analysis of

the effects of habitats on communication systems, the role of communicative interactions in the solution of ecological problems, the ergonomics of social signals, and the relationship between social interactions and population genetics.

Non-Verbal Communication reflects the current state of research on social signals in man and animals. For the student of non-verbal communication, there is still an abundance of fundamental problems, unrecognized and unsolved.

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Automotive Problems

Alternatives to the Internal Combustion Engine: Impact on Environmental Quality. By Robert U. Ayres and Richard P. McKenna. Pp. xvi+324. (Johns Hopkins: Baltimore and London, August 1972. Published for Resources for the Future, Inc.) £5.40.

THE publication of this carefully prepared study is most timely and it deals very thoroughly and in a positive way with the most ubiquitous, and in urban areas by far the worst, source of atmospheric pollution—and the one which is probably the most difficult of all to eradicate. In spite of its title, however, the authors have, perforce, devoted much of their space to discussing existing internal combustion power plants rather than their replacement.

Although this is a well-expressed and readable book, a modest knowledge of basic physics, chemistry and mathematics is necessary to its full understanding. The mere mechanics of engines are clearly explained, although a few more, and better detailed, drawings to supplement the diagrammatic ones would have improved these parts of the book. In particular, the confusing Wankel (and similar) "rotary" engines could be better illustrated for those to whom this cycle is new. I have always doubted if these rather involved engines, with their complicated wearing surfaces—far more difficult to renew than a simple cylindrical rebore—will ever replace simple reciprocating motion acting on a revolving shaft; while their reduction of pollution must be problematical.

Whatever the internal combustion cycle may be, the only answers to pollution must lie, first, in assuring the most complete combustion possible and, second, in thoroughly cleansing the exhaust, without undue back pressure, of waste products before releasing it to the atmosphere. The first is advantageous to the user of any vehicle; but the second adds cost, bulk, weight and complicated equipment requiring maintenance and replacement. The US Congress has advised the American automotive industry that a very low-emission vehicle is expected in five years—only to be told it is impossible. Technically it is not impossible; commercially, in these days of virtually throw-away motor cars, it is probably not viable. After all, the modern simple exhaust silencer is so shoddy that it rarely lasts over two years. Not only the essential maintenance of a true exhaust cleaner at 100 per cent efficiency but the policy for thousands of millions of older vehicles present terrific problems.

The chapters on the internal combustion engine are good and give the general reader a clear picture of how the pollutant products of combustion are generated—particularly the destructive nitrogen/oxide compounds—and the effects of fuel quality and additives, for example sulphur content and tetraethyl lead respectively.

It is when discussing alternatives to the internal combustion engine that the enormity of the problem is revealed in all its bleakness: the weight, bulk and restricted performance of the best of them, electricity; the many problems of the steam engine; and the early evolutionary state of the fuel cell. Here the authors have presented a good and well-balanced picture of this somewhat unpromising field. Developing practical alternatives is going to be very expensive; but there is a silver lining to the gloom in that maintenance costs may well prove substantially lower, although first cost and most fuel costs will be higher. There is, too, the vast inertia of the place of the internal combustion engine in modern life—with its vested interests and political lobbies—before it can be superseded. One thing is certain: the internal combustion engine is inevitably doomed, for, if its emissions are not cleansed, it could choke its users to death; but more certainly still, there is a finite limit to the vast stores of hydrocarbon fuels which are being so lavishly squandered in this twentieth century. The harnessing of the Sun's heat in vegetable alcohol—which would allay pollution in its growing—could scarcely be on the scale needed by the overcrowded world of the future.

For those who want a sound résumé of the background to the part being played by road vehicles (and of the

state of the art for possible palliatives or alternatives), this is an excellent and authoritative, comprehensive reference.

JAMES HAY STEVENS

Elmer Sperry

Elmer Sperry: Inventor and Engineer. By Thomas Parke Hughes. Pp. xvii+348. (Johns Hopkins: Baltimore and London, November 1971.) \$15; £7.15.

THIS is a comprehensive and well-written story of a remarkable American who, born in November 1860, spent an incredibly active life in patenting and exploiting over 350 inventions through a most unusual combination of technical and commercial skills.

His father seems to have taken little interest in him as a child, and as his mother died within a few hours of his birth he was brought up in the home of his grandparents at Cortland, a village twenty miles north-east of Ithaca. Here, while attending the local school, his interest in all things mechanical or electrical made him a welcome visitor in the local engineering concerns. The Cortland Waggon Company turning out thousands of horse-drawn vehicles annually was destined to play an important part in young Sperry's life during the next few years.

The name Sperry is of course mainly associated throughout the world with the numerous practical applications of the gyroscope but the author has shown that in his early years he played a prominent part in the development of electrical engineering in the United States, a fact which is often overlooked. At a time when Edison, Elihu Thomson, Houston and others were making great strides in this field, Elmer Sperry in his teens was producing original ideas in the design of arc lamps and generators and carrying out extensive distribution systems with financial support from established industrialists convinced of his originality and flair for putting his ideas into practice.

About this time he made contact with a professor at Cornell University and helped him in the construction of a Gramme dynamo, with the result that he made his first invention, a governor to control the voltage of the machine, and soon afterwards obtained his first patent. Within a few months he was making an intense study of all electrical patents as they came out and applying his teeming brain to improvements on the ideas disclosed. He soon discovered, however, that there was much more in the patent field than the ability to design and construct: he became involved in a legal battle with one of the rapidly growing competitors. He came out "on top", however. He was successful at 23 in securing support to found a company

bearing his name with a capital of a million dollars with a salary and proportion of the profits. Major projects followed, an outstanding example being the original Chicago central generating station. This was accompanied by a spate of trouble with the telephone authorities through leakage from his underground cables and at 25 he was calling together other undertakers to form NELA for pooling these problems.

At 27 Sperry found time to marry and in due course produced a family of three boys who became distinguished successors to their father and a daughter who cooperated in his patent activities. His interests spread rapidly and magnificently through street cars, electric locomotives, gas engines, electrically propelled vehicles and, on an even grander scale, the growing field of electro-chemistry.

It was not until he had reached the age of 50 that Sperry became interested in the developments of the gyroscope. He formed one more company, then restricted his time to research and development. He became young once more and through his energetic efforts created a monopoly resulting in 10,000 dollar orders from the American and other navies for his sophisticated and original gyrostabilizer and gyrocompass. The account of his timely entry and success in this field reads like a fairy tale but the chapter devoted to the theory of the various devices would alone constitute an excellent introduction to the subject for any student aiming at a doctorate.

No reviewer of this delightful book could avoid paying a high compliment to the author for the wealth of illustrations which he has succeeded in bringing together and for his enthusiasm which comes through on every page and appropriately matches the great character of his subject. PERCY DUNSHEATH

Sound Made Visible

Seeing Sound. By Winston E. Kock. Pp. vii+93. (Wiley: New York and London, February 1972.) £3.75.

THE title and a quick glance at some of the photographs in this book excited considerable interest but on closer inspection I found it extremely puzzling and I cannot really envisage the readership for which it is written. This does not mean to say, however, that it is of no value. The first chapter is a very elementary introduction to the nature of sound and then in chapters 2 and 3 we meet perhaps the most interesting section describing a simple microphone-lamp method of scanning a sound field and making the shapes of radiated beams, or of interfering beams, literally visible in a photograph. The technique

of revealing phase differences is particularly interesting, though simple, and the photographs illustrating this chapter justify the title of the book quite well. These chapters might form a useful source of ideas for student experimental projects. Chapters 4 and 5 are fairly conventional accounts of sound-spectrograph techniques used in the study of speech. They are copiously illustrated and would provide a useful source of illustrations for general lectures. Chapter 6 is devoted to a mixture of items which, on the whole, are more concerned with developments in electronic organs than with visual presentation and the final chapter skims rapidly over auto-correlation functions, acoustic holography and the directive properties of loudspeakers.

The overall impression is thus that some very good ideas and quite a number of excellent and interesting photographs have been made the excuse for a book but the remaining material added to make the size acceptable does not come up to the same standard and is treated somewhat superficially.

C. A. TAYLOR

Unobjective Test

Objective Personality Assessment: Changing Perspectives. Edited by James N. Butcher. Pp. x+212. (Academic: New York and London, August 1972.) \$6.95.

THIS book, we are told, "will be invaluable to all those who work with personality testing—including psychologists, clinicians, psychiatrists, educators, sociologists and social workers—as well as to researchers in the field of personality". Not so. It is difficult to think of anybody who would really benefit very much from reading this slight and misleadingly entitled volume. The book does not deal with objective personality assessment at all; it is concerned essentially with the Minnesota Multiple Personality Inventory (MMPI) and should have been entitled accordingly. Five of the seven chapters published in this volume are based on invited addresses at the Fifth Annual Symposium on Recent Developments in the Use of the MMPI; thus the book sails under a false flag, and readers attracted by the title will be sadly disappointed—even though such contributors as Meehl can hardly help writing interestingly and well, and producing thought-provoking comments.

The MMPI is certainly a widely used test, and if one had to choose between it and such projective measures as the Rorschach, TAT, or the reading of tea leaves, one would probably come down in favour of the MMPI. Yet the reason for its existence has always been

somewhat shrouded in mystery. Certain groups of items correlate rather poorly with unreliable and highly subjective psychiatric diagnostic judgments made by Minnesota psychiatrists; thus were the diagnostic scales created. They do not work very well in Minnesota as far as predicting diagnosis is concerned; they work even less well in other American States, and they do not work at all well in England and Germany. The reason of course is largely that psychiatric diagnoses have very little meaning in themselves, and are hog-tied to local myths. It has been shown that one and the same patient is five times as likely to be diagnosed "schizophrenic" in New York as in London; it would be much easier to predict his diagnosis from the nationality of the officiating psychiatrist than from a knowledge of his MMPI score.

Even if the diagnostic system were meaningful and reliable, and even if the MMPI correlated more highly than it does with diagnosis, would the results really be of any great interest? There is only a poor correlation between diagnosis and treatment, and after all it is the treatment, and decision concerning it, which is important. This book does not come to grips with the real problems of the MMPI, just as little as it even mentions truly objective personality tests. It is very doubtful whether it will be of any interest to English readers, who in any case have not followed the American practice of using the MMPI very widely.

H. J. EYSENCK

Statistical Questions

Statistics: a Guide to the Unknown. Edited by J. M. Tanur, F. Mosteller, William Kruskal, R. F. Link, R. S. Pieters and G. R. Rising. Pp. xxiii + 429. (Holden-Day: San Francisco and London, May 1972.) \$11 cloth; \$5.50 paper.

WHAT statisticians do and the frequently ingenious ways they go about their mysterious work is the subject of the forty-four essays that make up this book. They have been written—each to a different user of statistical method—and assembled to acquaint the general reader and especially the student with the part played by statistics and probability theory in the modern world. The essays were commissioned to this end by a joint committee of the American Statistical Association and the National Council of Teachers of Mathematics.

With the exception of a few that are either patronizingly elementary or excessively obscure, each of these essays is a kind of teaser. With an

average (mean, that is) space of rather less than ten pages to work in, none of the essayists has been able to tackle more than a single facet of one limited aspect of a tiny problem within his special field.

So what we have in the best of these papers is something between a detective story and a riddle. A problem is stated, its significance is established, the hopelessness of ever finding a final and definitive answer is indicated . . . and then the reader is permitted to follow the statistical worker through the mass of indeterminates to an imperfect but often useful solution.

It seems reasonable to assume that such a reading experience could have value in motivating the university, or younger, student in his first statistics course. This is important because, in the United States at least, there are few subjects in the curriculum that produce more traumas.

Besides showing something of how a statistician works and what he is good for in the world, our essayists offer many interesting bits of statistically derived information. They ask and then answer questions such as the following: Why is it that although samples of a few dozen or a few hundred cases are often used in research, including biological, a sample of 40,000 children proved worthlessly undersized in a test of Salk polio vaccine? How is it that a hospital with an overall postoperative death rate of 6.5% may not be a more dangerous place to take your trade than one showing a rate of only 0.25%? Why, with many authorities accepting as proved a causal relationship between cigarette smoking and early death, do some statisticians remain unconvinced? Is it true that people, and most particularly famous ones, are less likely to die in the months before their birthdays than in the months that follow? (Most interesting, of course, is what this apparent fact suggests about the power of the will to live.) How has it been demonstrated that the odds are many, many millions to one that the absence of women among the jurors who tried Dr Benjamin Spock was no accident?

How can internal evidence, by itself, tell us which statesman of long ago composed which historical document? Why is it that when airlines use sampling methods to apportion revenue among themselves every one of them gets more than when precise, detailed accounting is done? Why is an estimate based on a comparatively small sample sometimes more accurate than a full census of the same population? Why is no cost-of-living index ever correct?

Since either it will rain or it will not, what does the weatherman mean by forecasting a "70% probability"?

DARRELL HUFF

Plants on Test Sites

Radioecology and Ecophysiology of Desert Plants at the Nevada Test Site. By A. Wallace and A. M. Romney. Pp. viii + 439. (US Atomic Energy Commission: Oak Ridge, Tennessee. National Technical Information Service, US Dept. of Commerce: Springfield, Virginia, 1972.) \$6.

A LARGE number of ecological studies has been carried out in the transition zone between the Mojave and Great Basin Deserts in southwest Nevada, which is the location of the United States Atomic Energy Commission's nuclear test site. The preliminary results of many of the botanical exercises are written up in this volume.

The book does not claim to be comprehensive in its coverage of the work being done at the Nevada Test Site, but it does contain a large volume of generally uncoordinated data both on the effects of chronic and acute exposure to gamma radiation on plant communities and also on general physiological studies of selected desert species. It is set out as a collection of fifty-three short accounts of the results obtained from the experimental projects currently being undertaken. Although there is a brief introduction which deals with the geography of the area, there is generally a lack of background information necessary for a reader unfamiliar with the area to be able to place the detailed papers in their true perspective. In particular, an account of recent land-use history of the area would have been most useful. An even greater gap in this book is the total absence of any general summary of the work, which might have drawn together the various lines of research. As it is, one is left with the impression of a series of detailed but disconnected research topics which have little in common except that they have been carried out in the same area. Even the order in which the papers are arranged does not seem to follow any system which, together with the lack of an index, makes specific data difficult to retrieve. Among the papers there is considerable repetition of the information supplied in the introduction.

The most useful feature of this volume is its extensive bibliography, covering many aspects of desert plant ecology and radioecology. The book is also useful as a source of experimental data on a variety of subjects, for example, the persistence of radionuclides in soil, plants and mammals, the direct relationship between nuclear volume and sensitivity to gamma radiation, and the patterns of nutrient cycling found in desert ecosystems.

This is a book which will be of value mainly to specialists in these fields.

P. D. MOORE

CORRESPONDENCE

Creation *versus* Evolution

SIR,—In your editorial entitled "Creation in California" (*Nature*, 239, 420; 1972) you ask for scientists to identify themselves who do not hold to the "doctrine of evolution" (your terminology), who are working in a field related to the evolutionary question. I am a research chemist presently involved in work on biological membrane structure and function, and formerly on polypeptide and protein conformations, and am ready to state my dissenting position.

I do not believe that the evidence in hand demonstrates that the origin and development of life on Earth occurred by an evolutionary pathway. My objection is not to evolution on a small scale, that is microevolution, meaning the variation and development within related plant or animal groupings; indeed, I would go so far as to say that in my opinion this type of evolution has been proven to occur. One is not warranted, however, to extrapolate from the evidence for microevolution to the contention that general evolution is therefore also true.

The simplest living cells are still very complicated on a molecular level; to suppose that there were even simpler cells in the past is pure speculation designed to fit a theory. In any case, it is generally agreed that the coming into existence of the first living cell must have been an extremely improbable event, but we know it happened because cells now exist.

Natural phenomena may be defined as events which have a finite probability of happening, on the basis of known or at least knowable natural laws. Events which are extremely rare or which have a vanishingly small probability of occurrence cannot readily be classified as natural phenomena. The origin and development of life fall into this category. It must be admitted that such events may be the result of a supernatural act, unless one wishes arbitrarily to dismiss the possibility that God exists and that He can and does operate the universe. It is my reasoned opinion that the empirical evidence in hand concerning the nature and origin of life is better explained in terms of

supernatural, creative acts than it is by the general theory of evolution.

The editor of *Nature* is to be complimented for providing a forum for the open discussion in scientific circles of the creation *versus* evolution question.

Your faithfully,

GARRET VANDERKOOI

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SIR,—Your challenge (*Nature*, 239, 420; 1972) to those who do not believe that evolution supplies all answers on the existence of the universe, Earth and life cannot go unanswered.

I do not see that the existence of the universe, the Earth, and living systems can be separated from one another: you seem to be considering only the Earth and species but ignoring the whole question of beginnings. Even such an impersonal idea as a "master clock-winder" who established matter, perhaps set up some master laws (including evolution?) and is, for all intents and purposes, now "dead" should deserve more than one sentence in a science textbook. A God who took a direct part in organization of the universe should deserve far more recognition. You state that the "doctrine of evolution . . . deserves to be called the truth." Jesus Christ said, "I am the Truth" (*John*, 14, 6). If evolution proves the non-existence of God, then Jesus was a liar and the dichotomy need not be reconciled. If evolution does not speak to that question, then the possibility that God could have taken an active role in organization of the universe, Earth, and life is a viable alternative.

A chemical kineticist will state that his proposed mechanism is consistent with the kinetic data but that kinetics cannot prove a mechanism, for there may be other mechanisms just as consistent with the data. Then one must look for additional evidence to support or disprove the mechanism. There are too many unanswered questions in evolution for me to be able to call it "truth", so

we must look for additional evidence. Two arguments will be presented.

If the essence of thermodynamics may be stated as given by Giles, "There exists an additive function of state S , called the entropy, and a set of additive functions of state, called components of content, such that, for any states a and b , $a \rightarrow b$ if, and only if, $S(a) \leq S(b)$, and $Q(a) = Q(b)$ for every component of content Q " (quoted by W. Brostow, *Science*, 178, 123; 1972), then simple examination states that something or someone larger than the universe must have formed the original state a from which entropy is changing to this day. I understand that information theory and the second law of thermodynamics have been redefined and expanded to allow for living systems to drive constantly toward greater organization in contrast to the rest of the universe (advance statement on Gatlin, L., *Information Theory and the Living System*, Columbia University Press, 1972). Without having read the ideas, this seems to give to living systems a new form of "vitalism" such that in only one aspect of life (evolution), different forms of the laws of thermodynamics apply. Is it not better to have a God that is beyond the limits of the universe because He made it? Are not evolutionary trees of protein and nucleic acid structures just as well explained by a common originator as by a common origin? If so, is not the alternative mechanism of relationship of the species worthy of presentation?

The second argument is lacking an exact figure. At the AAAS meeting in Philadelphia last December, it was stated that it should be possible to synthesize more efficient proteins because there are 10^{72} (? I did not record the number) electrons in the universe and 20^{100} possible combinations of amino-acids (not to mention modification of specific residues by methylation, acetylation, phosphorylation, and so on) in a protein containing 100 amino-acid residues (one of the smallest proteins) so that evolution had to stop with a combination that worked and not necessarily the best (comments in parentheses are mine, not the speaker's). If there

are 10^{10} to 10^{12} different kinds of protein molecules (Lehninger, in *Biochemistry*, 5, Worth Publishers) the probability of getting that many functional proteins with the limited material available would appear to be infinitesimal or at least such that reasonable doubt may be expressed that probability is the only answer. Intervention by God to make functional protein and nucleic acid is not disproved by these statements.

It would appear that the cardinal question is, "Is there a God?" If not, how can we prove that scientifically? If we cannot prove the absence of a God, then we must consider the possibility that there is a God and He could certainly also have the power to be active in the universe (a far more positive apology for the existence of God and what He means to individuals can be given, but I see the issue in your editorial as whether anyone in your readership feels that there is reasonable doubt that evolution supplies all the answers to a scientific mind). In my opinion there is sufficient doubt such that the California Board of Education cannot be told that "evolution is . . . the truth" and no other alternatives need be considered.

A far more consistent universe may be studied if we are not left to the vagaries of probability but rather can study the works of an unchangeable and sovereign God. Such a basic philosophy concerning the universe can make for a more meaningful science, limited only by man's ability to discover and rightly interpret the works of God.

Yours faithfully,

HAROLD VAN KLEY

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SIR,—I am a taxonomic botanist and have been for the last ten years.

I was at one time a convinced believer in evolution, but that was before I studied the subject with a critical eye. I now reject the whole thing as an utter tissue of lies, and have come to accept the fundamentalist position of divine creation as being the only possible explanation of origins—that is, with purpose and destiny, as opposed to blind meaninglessness, which is what evolutionism presupposes.

Too late was my attention drawn to your generous offer (*Nature*, 239, 420; 1972), but I am not so concerned about obtaining free copies of your journal as I am about making known God's truth.

May the light of God's holy word shine in upon and illuminate the souls of those whose understanding has been darkened by the specious arguments of

the Devil, and may the sons of men sing praises to their Creator and Redeemer instead of paying lip-service to Satan.

Yours faithfully,

ALAN RADCLIFFE-SMITH

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SIR,—With reference to your editorial comment "Creation in California" (*Nature*, 239, 420; 1972), though not a convinced creationist, and certainly not a supporter of the more extreme forms of creationism, I do want to defend the more moderate creationist views, which I think are scientifically tenable and so deserve to be considered alongside evolution.

Theories of the origin of life can be classified under three headings: naturalistic evolution, theistic evolution, and creationism. It is difficult to think of any scientific test to distinguish between the two evolutionary views. Creationists make postulates about the past that can be checked by the evidence and which differ from evolutionary postulates. All creationists hold to a polyphyletic view of the origin of life—a view that an evolutionist has shown to be scientifically tenable¹. They believe in the original creation of a multitude of "kinds" of organisms (the term is not synonymous with "species") which have since undergone some speciation. Thus they predict the existence of gaps in the fossil record between the remains of the older forms or organisms, as is the case². Some creationists believe that there was only one creative act, which occurred some tens of thousands of years ago. Others hold that there were a series of such acts spread over hundreds of millions of years. These postulates can be tested. The first is at variance with radioactive dating (which its adherents regard as erroneous), but the second is in accord with the datings and the fossil record. Thus the more moderate creationist views are just as compatible with the evidence as the evolutionary views.

The present antipathy towards all forms of creationism arises, at least in part, from the invasion of science by an extra-scientific assumption—namely the belief that the universe is a closed system which cannot be invaded by the supernatural. This view was not held by the founders of modern science, most of whom were devout Christians, and is not an axiom essential for the practice of science (the essential axiom is that the supernatural interventions are non-capricious), nor is it deducible from the success of science (as fallaciously argued by Hume). Scientific investigation can only show whether or not a particular event was genuinely

supernatural, not whether supernatural events in general can or cannot occur. The influence of this philosophical prejudice in science is well illustrated by the readiness to accept the idea of creation as a "natural law", as in the original steady-state cosmology, whilst rejecting the idea of creation as an "act of God". If it is objected that "acts of God" have no place in science, the answer is that if one chooses to hypothesize about the origin of things one must become unscientific in that origins are once-for-all happenings that cannot be experimentally verified. At best theories of origins can be falsified by comparison with the circumstantial evidence available. Also, if a creator-god exists, then his creative act will be met at some point, if not the origin of life, then the origin of matter/energy. All this shows the importance of clearly stating one's philosophical and religious position when postulating theories of origins, since this inevitably influences such theories.

One final comment. Why was your "challenge" limited to university scientists? Are industrial scientists an inferior breed? Maybe they are freer thinkers as a result of being less affected by the pressures for conformist thinking that exist in universities. As a point of information, the Creation Research Society, one of the lobbies in California, has a few hundred members, all with higher degrees in science.

Yours faithfully,

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¹ Kerkut, G. A., *Intern. Ser. Monogr. Pure App. Biol.*, 4 (Pergamon, London, 1960).

² Simpson, G. G., *The Meaning of Evolution*, 209 (Bantam Book, New York, 1971).

SIR,—I agree with the basic points you raised in your editorial on "Creation in California" (*Nature*, 239, 420; 1972). Being a former resident of that allegedly super-sophisticated state, I have witnessed first hand the emasculation of scientific truths in education that you spoke of. To set the record straight, the non-fundamentalist educational authorities of that state are just as much to blame for the institution of these measures as the "special creation" advocates. Take, for instance, the present Superintendent of Public Instruction there, whom I personally heard bluster: "Any method of teaching reading is as good as any other." Presumably he extends this eclectic approach to interpretation of biological facts as well.

The arguments of the special creationists usually involve irrelevant

attacks on supposed shortcomings of current astronomical, geological and evolutionary theories, combined with fallacious appeals to authority (especially "scientific" authority!) and mawkish appeals for public sympathy for the religious people of California.

The fact that these men of blind faith will probably succeed in keeping from our children the truths of science, that have taken so many great men of the past so many years of intellectual effort to develop and prove is, in my opinion, the lesser evil of this whole affair. What is far more objectionable is that they are undermining the self-confidence of young minds by teaching them that they can never be sure of any conclusion they might reach on

any subject: there might always be some (unspecified) facts they have missed, or some people's feelings they have hurt, or some movement's sacred beliefs they have transgressed. This lends credence to the dogma of Subjectivism, which substitutes an opinion poll for cold, hard scientific truth. The degree to which such a dogma will destroy the scientific spirit in our young children is the degree to which it will reap its ultimate evil consequences.

The one area where the special creationists actually have justice on their side lies in their claim that they should be given some "democratic" control over public education, because it is their money which is supporting it and their children who are being in-

structed under it. As long as public schools, supported by coercively obtained tax moneys, run by the government, and dedicated to the principle of egalitarianism, continue to exist in this society, no one will ever have a conclusive rebuttal to such an argument. But public education does not have to continue to exist. And when it is abolished in this country, as I hope to see one day, education will no longer serve as a political football, to be tossed about first by one pressure group, then another.

Yours faithfully,

DAVID SOLAN

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Obituary

Dr John Hutchinson



THE death of Dr John Hutchinson on September 2, 1972, removes a great figure from the British systematic botany scene and perhaps marks the passing of a long and honourable tradition in this field—that of the "generalist". To find a parallel to Hutchinson's breadth of experience in examining and knowing his plants, both alive in the field and preserved in the herbarium, great names from the past such as Hooker, Bentham, De Candolle and Engler have to be invoked.

Born at Blindburn, Wark-on-Tyne, Northumberland, on April 7, 1884, Hutchinson worked for a while in local gardens and then attended evening classes in botany and drawing at Rutherford College, Newcastle.

In April 1904 he entered Kew as a member of the gardening staff under

Mr W. Dallimore. His ability caught the attention of Mr W. Watson, the then Curator, who recommended him to fill a post as temporary assistant in the herbarium. Here he had the guidance of a wise and able botanist, Dr Otto Stapf, to whom he was for several years a personal assistant.

He held the posts of Assistant for India from 1907 to 1909 and from 1916 to 1919, and Assistant for Africa from 1909 to 1916. In 1919 he took charge of the African Section in the herbarium, a post he held until 1936 when he was appointed to the Keepership of the Museums of Economic Botany at Kew, from which he retired in 1948.

First and foremost of Hutchinson's achievements is his contribution to phylogeny and to general plant classification. The publication of the first edition of the *Families of Flowering Plants* was a major event in the history of phylogeny in Britain. The first volume, containing the Dicotyledons, appeared in 1926, and the second, containing the Monocotyledons, in 1934. British botanists were presented with a new and original scheme of plant classification based on theories of probable phylogeny and taking into account the important researches of Bessey. As Lawrence has written, Hutchinson's classification "has been a greater stimulant to phyletic thinking through the past decade or two than any other similar contribution".

In 1959 the eagerly awaited second edition appeared, with little change in the Monocotyledons, but with the Dicotyledons drastically rearranged and indeed reflecting a new theory about their evolution. In the first edition they had been primarily divided into the Archichlamydeae and

Metachlamydeae, following the freedom or connation of the petals. In the second this was superseded by a new division based on habit. The new theory postulated an early evolutionary dichotomy into two groups, one of families primitively and predominantly woody, the Lignosae; the other of families similarly herbaceous, the Herbaceae.

It would not be true to say that this scheme has commanded general acceptance, but, like the first edition, the second acted as a wholly beneficial catalyst, setting people thinking and discussing.

In 1964 came the first volume of *The Genera of Flowering Plants*, with a second in 1967. These mark the start of an undertaking so huge that it would surely have daunted anyone less confident and enthusiastic than Hutchinson. Inspired by that great classic of the last century, the *Genera Plantarum* of George Bentham and Sir Joseph Hooker, this new work projected a complete descriptive account of all the known genera of flowering plants. A third volume is in an advanced stage of preparation, but the work as a whole is incomplete.

Particularly in his earlier years, Hutchinson carried out careful revisional studies of various families and genera. He contributed, for example, the families Euphorbiaceae (in large part) and the genus *Ficus* to *The Flora of Tropical Africa* (1912 to 1917). These are lengthy and accurate monographic studies and rank among the best of the many fine revisions in that great collaborative work. He also had a lasting interest in the genus *Rhododendron*, to whose classification he made major contributions.

In 1927 to 1936 he, jointly with Dr

J. M. Dalziel, produced *The Flora of West Tropical Africa*, a two-volume work describing the 6,000 species then known for a vast area of western Africa. This flora set new standards, being based mainly on a system of keys rather than formal descriptions, and served as a model of conciseness for a number of subsequent floras.

Many people owe their interest in plants or botany to Hutchinson. He was adept at popularization, writing easily and clearly, and having the unusual gift of being able to illustrate his own works. In 1945 his little book, *Common Wild Flowers*, was published in the Penguin series, and further volumes, *More Common Wild Flowers* and *Uncommon Wild Flowers*, followed in 1948 and 1950. In 1948 he published (with Dr R. Melville) *The Story of Plants*.

His skill as an artist has already been mentioned. Most of his work was in black-and-white, executed fluently, accurately and with considerable artistic merit. Some of his artistic work has been exhibited internationally.

Deservedly, many honours were bestowed on him, including an honorary Doctorate of Laws at the University of St Andrews in 1934, Fellowship of the Royal Society in 1947, the Victoria Medal of Honour of the Royal Horticultural Society, and the Linnean Gold Medal of the Linnean Society of London in 1965. This year his eminence was recognized by the award of the OBE.

In assessing his achievements various points stand out: firstly, his extraordinary productivity; his major works alone cover more than 6,000 pages of print. Next, perhaps, is the way in which he continued to work after retirement in 1948. Probably the major part

of his published work dates from after that time. Indeed, on the very day of his death he worked in the Kew herbarium which had for so many years been his working home.

In spite of his activity he always had time to talk to and encourage younger people. His kindness to others and his innocent and endearing delight in his own achievements will not easily be forgotten by those privileged to know him.

Academician P. A. Reh binder

THE recent death of Academician Reh binder, the Soviet Union's most eminent colloid scientist, will be sad news for those who had the privilege of meeting him. He died at the age of 74, following a heart attack, while still at the height of his intellectual powers, having led his country's research in disperse systems for nearly 50 years. Over this period he published, with a long series of colleagues, about 500 scientific papers, and his students included some 30 full "doctorates" and 100 "candidates" (comparable with PhD).

Academic Reh binder was an early pioneer in the fundamental study of the flotation of minerals—but he is nowadays best known for the "Reh binder Effect" and for systematic studies of the rheological properties of disperse systems. The Reh binder Effect is the reduction of strength of solids by the action of surface-active substances. Originally this effect was interpreted simply as a lowering of surface energy by adsorption and hence reduction of the work required to produce new interface; but it must also involve the creep-

age of surfactant down grain boundaries or dislocations while the body is under stress. Examples of the effect are claimed to occur with materials as diverse as pure metals and various types of rock (under the action of rock drills, or during crushing and grinding).

Reh binder's researches in recent years were particularly directed to studies of the "physico-chemical mechanics" of dispersions of practical interest such as clay slurries and cement pastes, for which purpose a range of measuring instruments was developed. Other interests included adhesives, emulsions and foams. This work was carried out partly in the Department of Colloid Chemistry in the University of Moscow, where Reh binder was professor (and, it is said, a brilliant teacher), and partly in the section of Disperse Systems at the Institute of Physical Chemistry of the Academy of Sciences of the USSR—a section which he headed from its formation to the time of his death.

Academician Reh binder was an indefatigable worker for colloid science, including amongst his activities the work of Editor-in-Chief of the *Kolloidnyi Zhurnal*. He was also the Soviet Union's representative on the scientific committee for the International Congress of Surface Active Substances. At the same time, he was a man of exceptionally wide cultural interests—a connoisseur of art, literature and music, and a fluent linguist in both French and German. No note of his life would ring true without a tribute to his personal charm, his encouragement for younger colleagues, and his earnest concern to develop friendly personal contacts with foreign fellow-scientists who visited Moscow or whom he met at congresses.

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